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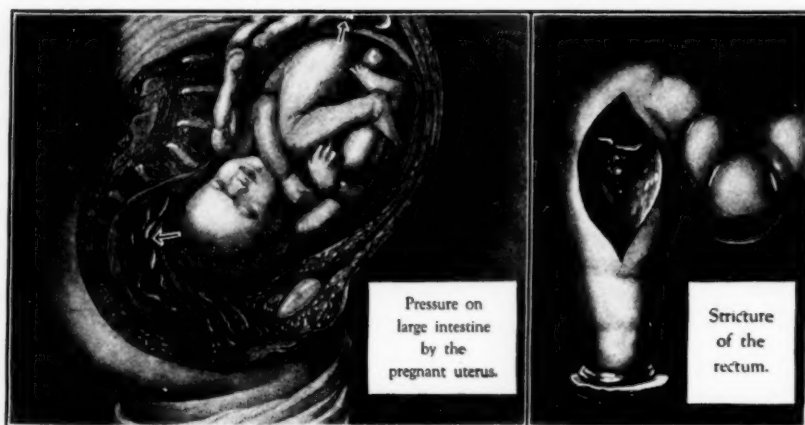
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Vol. LII, No. 11

New York, November, 1924

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A Further Study of the Toxic Effects of Local Anesthesia*

EMIL MAYER, M.D.,
New York

The great importance of local anesthesia is our special fields of medicine has induced your program committee to request a further review of the subject, together with such comments as may be advisable, adding reports of further fatalities in the hope that there may be a full discussion, that individual views may be freely expressed and conclusions reached that will result in the saving of human life.

Too much credit cannot be given to the Therapeutic Research Committee of the Council on Pharmacy and Chemistry of the American Medical Association, who give so generously so much of their time to the scientific problems of the Association, for instigating this research. They have been most helpful in every way to our committees, responding eagerly to any demands made upon them, advising, appreciating and encouraging at every step.

The Board of Trustees of the Association have realized the importance of the investigation, have cheerfully endorsed it, granted the necessary funds for its completion and expressed their gratification at its successful outcome. Their loyal support is deserving of the highest praise and gratitude of the profession.

The individual members of the various committees have cheerfully accepted any detail assigned to them, have loyally given the best that was in them, devoting many hours unselfishly to the success of the investigations without recompense, save the knowledge of a duty willingly performed, and have worked in unison for the common good. As chairman of these committees I beg to extend to them my grateful appreciation and to assure them that I have been honored by association with them and have been profoundly touched by their many kindnesses, endorsement and unanimous support.

* (Read before the American Academy of Ophthalmology and Otolaryngology at its 29th Annual meeting in Montreal, Canada, September, 1924.)

No small share of our appreciation belongs to those members of our profession who, relying on our honor, have given us their confidences and have related with meticulous care the details of the tragic occurrences they were unhappily forced to witness.

At the outset we were met with the fact that we were entering on a subject that had never before been attempted. It became necessary to devise means for conducting the investigation, and to plan for its study from every point of view. Mere tabulation would have been futile. We had to ascertain if deaths occurred, the drugs responsible, the symptoms of poisoning, and this included measures used to resuscitate as well as any contributory factors; a study of any necropsies, animal experimentation undertaken when necessary and finally, after careful study, the presentation of such recommendations as would tend to lessen the number of fatalities and result in saving human lives. Two reports were published by my former committees.

The report of the general Committee was published as a special article in the *Journal of the American Medical Association* (March 15, 1923) and given ten pages of its valuable space. It has been widely abstracted, commented upon editorially in many Journals; many letters and personal expressions have been received, all of them of hearty approval. This report, preceded by an analysis by the chairman of the Therapeutic Research Committee, Professor Torald Sollmann, was submitted first to the members of his committee, approved and signed by each. They expressed their appreciation of the work. The keynote of their analysis was in the statement that "the committee believes that the observance of the concentrations recommended will reduce the risks materially, if not altogether. This does not question the right of the surgeon to depart from these concen-

trations. But he may see that in departing from them he is treading on dangerous ground."

Pharmacological studies became a necessary integral part of this investigation and these were conducted for us in the laboratory of the department on Pharmacology, under the direction of Dr. Hatcher. In the annual summary of work done in that department of the Cornell University this entire report appears showing the result of studies made in that institution and with its cooperatoins.

What the Investigation Has Accomplished

We have established the fact that fatalities occur more frequently than the occasional reports in medical journals would seem to indicate; deaths ascribed to a local anesthetic are frequently due to entirely different causes, often one drug is held responsible where others far more toxic have been combined. We have presented a new nomenclature. The symptoms preceding death have been clearly elucidated as well as the rapidity of their occurrence; we have therefore concluded that only such deaths as occur within a few minutes with the same symptoms, convulsions, paralysis of respiration and coma immediately after injection, are true anesthesia deaths. There seems to be an equal sensitiveness to these drugs in the throat, nose and urethra. The amount of epinephrin used has been carefully considered and further studies of this drug have been recommended.

Injection of cocain in the mucosa or subcutaneously have been found to be very dangerous. The use of cocain paste has been condemned. (This has created some criticism as will be shown later on.) Certain specific instances, as the presence of trauma and constitutional diseases are mentioned, as adding to the danger.

Generally speaking it was found that there was a tendency to use far greater concentrations than were recommended by the manufacturers or originators of local anesthetics, and each operator followed his own inclinations, no official statements as to the strength of these drugs had ever been made. This latter the Committee has attempted to supply.

We believe it to be indisputable that local anesthesia may be induced safely by the methods recommended by the Committee. The committee does not maintain that anesthesia may not be induced in other ways with comparative safety, but it does maintain that death follows their use in the ways that it condemns.

Toxic effects have come on so suddenly that a great variety of drugs have been used to resuscitate. Those likely to be of service have been mentioned.

While the reports have been most kindly received there have been criticisms. The 1920 report was thus criticized:

"In the entire experience of all the members of the Committee of the American Medical Association to investigate local anesthesia in nose and throat work there has not been a fatality, yet they accept scattered reports of deaths from local anesthesia from men, probably of much less experience than themselves, and in which there is no mention of a single autopsy having been performed, to eliminate the other causes of sudden death." To this I replied:

"While it is true that all of the three members of that Committee had no fatality, this was mentioned to indicate their freedom from bias in recording fatalities, no inference could be drawn therefrom that it was their skill that prevented such an occurrence. The confidential nature of communications prevented

the Committee from stating who had the unfortunate experience, but it should be stated unequivocally that death occurred often in the practice of men fully as prominent and as competent as any of the Committee, indeed they were known to us as especially capable men of large experience.

As to the statement, not a single autopsy, attention is called to the fact that this is untrue. That report mentioned *three autopsies*, cases 3A, 3B and number 10."

Criticism of this sort involving gross misstatements are vigorously resented. The critic was informed by me of the facts. He has had neither the grace nor the courage to retract his statements.

Cocain Paste

The recommendations as to the use of cocain paste have occasioned protest by some of the small minority who favor that procedure, notably among our confrères in England. Their statements are herewith appended:

Among the many journals reviewing the final report was the London *Lancet*, May 17, 1924, page 871.

After presenting the recommendations of the Committee editorially, the *Lancet* states that the present report will have done a very useful service if it results in fixing of recognized limits for the concentrations and doses in which local anesthesia should be employed. (The following communications were published in the same journal in May 31, June 7 and June 14th.)

(*Lancet*, May 31, 1924.)

"Sir:—Among the recommendations of a committee of the American Medical Association (see the *Lancet*, May 17th, p. 1013), is the injunction "that cocain paste should never be used," presumably because of its toxic effects. I do not know what exactly the committee means by "cocain paste," but if it is the mixture that goes by the name in London, then I, for one, am at a loss to understand the downright condemnation. The paste, as we use it, consists of pure cocain hydrochloride powder mixed with solution of adrenalin (1-1000) to the consistence of a mud or paste, and I have been employing it regularly five or six times a week for the last ten years as my mainstay in anesthetizing the nose, during the whole of which period I have not had one single case of cocain poisoning, not even of the milder variety. This claim may seem surprising, and the surprise will not be lessened when I say that in the septum resection operation I habitually use as much as from 4 to 6 grains of cocain for each case. Not only so, but when a general anesthetic is being used for that operation, I apply the paste to the septum as a preliminary, and my anesthetist friends can testify that that step is followed by no evils.

The secret of safety seems to lie in the judicious use of Adrenalin. In making his application let the surgeon paint the mucosa first of all with pure adrenalin solution alone and undiluted, in order to induce the local anaemia that minimizes absorption, and, at the same time, denudes and exposes the sensory nerve-endings. Then the cocain-adrenalin paste may be safely applied. It may, I think, be laid down as a rule that cocain whether in powder or in solution, should never be employed without adrenalin. The paste is, we must remember, not suitable for all purposes. I have never ventured to use it in the pharynx for example.

I am, Sir, yours faithfully,

DAN MCKENZIE.

On the following week this letter was published:

(*Lancet*, June 7, 1924.)

Sir:—Dr. Dan McKenzie deserves our thanks for setting forth his method of using pure cocain hydrochloride powder mixed with sufficient adrenalin solution to form a mud or paste for direct application to the nasal mucosa for producing local anesthesia. Further that thus applied he habitually uses as much as 4 to 6 grains of cocain for each case and never has he had one single case of cocain poisoning. Assuming that this is the cocain paste referred to in the report of the Committee of the American Medical Association which they recommend should never be used, Dr. McKenzie's letter is a timely and authoritative protest against such sweeping general statements.

Yet it seems curious to find that Dr. McKenzie goes on to lay down a rule that cocaine whether in powder or in solution should never be employed without adrenalin and the need for caution in the exhibition of all local anesthetics must be borne in mind, nevertheless many of Dr. McKenzie's friends regard it as one of the unrivalled advantages of cocaine, over all other local surface anesthetics, that cocaine may be used for light anesthesia of the nasal mucosa without the disadvantages occurring from the application of the adrenalin. Probably Dr. McKenzie referred to the use of strong application for pronounced degrees of anesthesia, but if this be so it only emphasizes the need for care in avoiding such generalizations as Dr. McKenzie himself condemns and which tend to hinder rather than help us in arriving at sound conclusions.

PATRICK WATSON WILLIAMS,
Clifton, Bristol.

This was replied to in the *Lancet*, June 14, 1924.

Sir:—If Dr. Watson Williams will turn again to my letter he will see that my reference to the advisability of always combining adrenalin with cocaine is preceded by the words, "It may I think," whereby I intended to convey the impression that it was a suggestion I was making rather than a rigid generalization, the perils of which I am aware of. For the point is, I know, one for debate. No one will, perhaps quarrel with the dictum that, as a rule, and particularly in extensive anesthesia, adrenalin should always accompany cocaine. In the matter of light anesthesia of the nose, however, as for examination purposes, the question is as Dr. Watson Williams quite correctly implies, not by any means so simple. In light sprays, adrenalin frequently sets up an annoying reaction, with sneezing and running at the nose, which I have known to last as long as forty-eight hours, and to be a cause of much bad language, directed, and with perfect justice, to the Rhinologist's address. But on the other hand, I have occasionally seen cocaine syncope follow even a trivial spraying of the nose with pure cocaine solution; the drug is then so rapidly and completely absorbed.

So there the problem stands, and each man must solve it for himself. Personally, I prefer a swearing patient to the fainting patient. But I am quite ready to admit that others may cherish the opposite opinion. May I add that syringing the nose with warm saline solution after adrenalin-cocaine spraying can, as a rule, but not always, be relied upon to avoid any subsequent irritation? It is curious that so violent a reaction seldom follows the more continued anesthetic applications needed for operation.

DAN MCKENZIE.

This criticism is quite different from the previous one. It contains a statement of the individual experience of men of undoubted ability and expresses their firm convictions.

There can be no quarrel here. They are entitled to their opinions while we reserve for ourselves the same privilege.

We were led to our conclusions by our knowledge of deaths reported to us following the use of cocaine paste. One undoubted death in the last series, two others occurring previous to the time specified in our investigations. As the sole contention of the *small minority* who advocate the use of this concentration was that it produced such profound local anesthesia that there could be no absorption and hence it was far safer than weak solutions, even absolutely safe, the bona fide deaths disproved their contention. Hence our decision.

We are not always in accord with our English confreres as regards therapy. We do not understand why Dr. McKenzie ever uses profound local and general anesthesia at the same time. This seems to be quite the custom in Britain for I note (*British Medical Journal*, June 21, 1924, 1091), a report of the meeting of Scottish anesthetists, which mentions "the use of local anesthesia after O'Malley's method and light chloroform with it." It assuredly is not the custom with us to use both forms of anesthesia at one time and the experiments of Dr. Macht reported by me in the *Journal* of March 15th states that the combination adds to the danger.

Our English confreres use chloroform very freely. We do not.

This subject far from being controversial must submit to the test of time. It is very interesting to note that Dr. McKenzie has seen serious toxic effects from small doses of cocaine, and never from the high concentration. Time alone will show which is right. In the meantime, speaking for my committee, we do not retract our condemnation of this dosage.

Substitution

The too frequent mistakes of substitution of one drug for another has been emphasized and suggestions made toward their prevention.

Unmeasured Quantities of Solutions

We have noted that unmeasured quantities of solutions, notably cocaine, are used. A cotton-tipped applicator is dipped into the bottle containing the solution and then applied to the parts. When asked how much they used, they do not know. If the solution would be placed in a graduated measure, the resultant liquid after the swabbing would enable the operator to say at least that not more than so many minims were used. The same argument applies to the spray.

Always Measure the Quantity of Any Solution Used

In order to secure further confidential reports of recent fatalities for further study and to answer any queries of members of the profession who may seek information on this subject, the Chairman of the Therapeutic Research Committee appointed a permanent Committee on toxic effects of local anesthesia to serve at no cost to the Association. Dr. Elliott C. Cutler, Lakeside Hospital, Cleveland, Dr. Robert A. Hatcher, Cornell University Medical College and Dr. Emil Mayer of New York have agreed to serve under the Chairmanship of the latter.

The Permanent Committee

Certain toxic effects and fatalities have come to our notice since the report was prepared and these are here presented. It may be stated that one fatality was published while the others came direct to me as chairman of the permanent committee. We have every reason to believe that by our reports we have stimulated the profession toward more frequent publication of accidents occurring in their practice; One writes, "my conscience will not permit me to withhold from you the history of the following fatality."

Among the communications received was the following: "I note your recommendation that 1% Procaine is advised. How do you reconcile this with the fact that dentists use 2% solutions?"

To this I replied that the opinion of the committee and that of very many men with whom we conferred was that 1% Procaine solution produced complete anesthesia; that one man of large experience invariably uses ½% solutions to the complete satisfaction of his patients and himself, and that I could not accept the dictum of dentists until they had shown by experiments that 1% was insufficient where 2% was successful.

Additional reports of toxic effects following local anesthesia have been received since January 1, 1924. These are in reference to:

A. Adrenalin

1. I was called hastily to see a patient who was dying from the effects of adrenalin administered by mistake.

2. I know of one death from that drug.

(Concluded on page 258)

Examination of the Urinary System

VICTOR COX PEDERSEN, A.M., M.D., F.A.C.S.

New York

Blood or pus in the urine or proceeding from the sexual organs must be traced to its source. If the genital passages may be ruled out in the examination, then the origin must be the upper urinary tract. As a preliminary procedure for the general practitioner in differentiating between the lower and upper urinary tracts no method is simpler or surer than the seven-glass test of the author which will be the subject of the next paper. In other respects one must consider the quality of the urine, the bladder, ureters, pelvis, kidneys, and the blood chemistry as making up the symptom complex. The quality of the urine must be considered from the physical, chemical, microscopical and bacterial sides. Of these the bacterial elements are much more apt to escape proper attention than the other data because the latter have been much more familiar among practitioners for many generations beginning with the original contributions of Bright.

In these days the equipment of laboratories is so vast and the delicacy and difficulties of tests so refined that no longer is it possible for the much engaged practitioner or specialist to do his own laboratory work. The day has passed for the "sight-and-sink" method of deciding the quality of the urine. Under the guidance of a suitably trained senior expert, experienced technicians now do this work in laboratories which occupy whole suites of rooms. We have lived through the time when albumin as such in the urine was regarded as proof of Bright's disease. During the epoch of this error many normal individuals with seminal or prostatic albumin were, for example, refused life insurance. The leucorrhoea of women may be washed away with the urine and give an imperfect reaction for albumin. Hence, in both sexes and all ages the one proper method of securing a specimen for a definite albumin determination is by catheter. In children the ureteral catheter may be employed when the urethral type of instrument is too large. Hence in the male one must be sure that we are not ascribing to renal origin albumin which definitely comes from the vesicles, prostatic urethra and bladder. The elimination of the bladder usually requires bilateral ureteral catheterization.

The remarks just applied chiefly to pus in the urine are equally true of blood, and it is a safe rule to lay down that a difficult case should be seen by the specialist during the period of activity and the period of quiescence after and between attacks. Only in this way can the direct influence of the attack be estimated in cases of indefinite low-grade symptoms interrupted by periods of excitation. In illustration of the points emphasized the following two cases are worth very brief notice. A patient in seemingly good health had visible blood in his urine for about two weeks. Because it was unaccompanied by subjective symptoms and seemed to be slowly improving, the patient was not brought to the writer for cystoscopic and other tests until the blood had ceased. When examined there was no definite cause discovered or discoverable in his bladder, ureters, or kidneys, or the function of the kidneys, or in the condition of the urine. Hence, as a preliminary investigation no decision has been reached, and the patient must carry the adverse chance if such there be until a relapse of the bleeding leads to recognition of its source.

The proper thing for the family practitioner in this case was to have brought the patient during the weeks of actual hemorrhage. This was the time when the determination of its source would have been positive and instant. A similar case was managed correctly, and the blood was seen to be coming slowly and surely from one ureter, so that the next step in the diagnosis was easy, namely the determination of tuberculosis of that kidney.

The meaning of pus in the urine is illustrated by the following case. Ten years ago the patient was seen for pus proceeding from an unhealed abscess in the prostate. Cystourethroscopy proved that the posterior urethra and prostate, prostatic sinuses, ducts, and the abscess were in the same condition as they had been when the office diagram of the case, still on file, was made ten years ago. The amount of pus, however, was rather larger than such a prostatic picture would produce. Hence, a seven-glass test was ordered, designated the upper urinary passages as also involved and followed by a cystoscopy and ureteral catheterization, which in turn revealed a badly diseased kidney on his left side, proceeding from the original prostatic focus.

Microscopy of the urine is never done today by sedimentation on the ground that in all important cases decomposition of the urine interferes too much with the correctness of the findings, especially if bacteria are present. The terms "pus" and "blood" in the urine, as employed in this month's contribution, mean sufficient quantity of these signs of disease to be real factors either on microscopic or macroscopic test. A few scattered blood or pus cells mean little or nothing. In this sense blood and pus are the expression of an actual process, and above all other considerations the bacteriology must be investigated. Decomposition which commonly gives bacteria especially those which do not belong to the disease opportunity to act, must be avoided. As in the previous article, the important aim is to reach a true and definite diagnosis of the bacteriology no matter what it may be. In other words, the absence of a suspected germ is not a finished diagnosis. Whereas the recognition of any pathogenetic germ is a complete diagnosis.

The basic origin of the pus may therefore be in either the sexual organs or the urinary organs as systems or in any one organ of each system. The primary index of the decision is the seven-glass test, whose technique is exceedingly important and will appear in a later contribution. Of course pus in the urinary organs requires cystoscopy ureteral catheterization and separation of the urine, and later the more advanced tests of blood chemistry, X-ray diagnosis and the like.

Epithelium in the urine is closely allied to pus in its significance. It may come from superficial middle or deep layers, each in the ascending order of its importance. A practical point is that epithelial desquamation as a new feature during the administration of the various formaldehyde urinary antiseptics means irritation by the antiseptic. The next step of this disturbance is usually red blood cells under the microscope or free blood in the urine. The indication is decrease or discontinuation or alteration of the antiseptic. As to the form of epithelium the

superficial cells are the predominant and the middle and the deeper layers the transitional types of the mucosa of origin. Hence the larger the number of transitional cells, the more profound the damage.

Casts are very familiar. The greatest meaning lies in the advanced and complex types always accompanying other signs of profound damage, for example—epithelial, waxy, pus, blood, fatty, crystalline, detritic and bacterial.

Crystals and amorphous deposit are often of little importance, especially if the urine has stood and begun to be decomposed, but in fresh urine if the crystals are very numerous and other signs of irritation are present, they may often indicate the type of calculus present somewhere in the urinary system. The distinction between turbidity due to phosphates, urates and bacteria in the urine is rather easily determined by the simple tests of Ultzman and Kidd, respectively following.

BOILING UPPER LAYER OF TURBID URINE IN A TEST TUBE

Turbidity				
Vanishes		Add acetic acid Increases:		Add acetic acid Remains:
Acid urates	Vanishes with gas: carbonates	Vanishes without gas: phosphates	Remains: Pus	Catarrhal mucus or bacteria

K O H ADDED TO TURBID URINE

Turbidity		
Vanishes	Remains:	Ropes up:
Uric Acid	Bacteria	Pus
	Stirring "swirls" bacteria	Pouring from glass to glass makes ropes

The details for searching bacteria which are present in pus and shreds in the anterior urethra of the male and the sexual organs of the female has been laid down in the first article. Repetition is worth while of the warning to carry the diagnosis to a definite issue and not to be satisfied with the absence of one suspected germ.

Cystoscopy requires much training and ability for practically painless performance and certain prerequisites for the benefit of the patient, as given elsewhere.¹

¹ A Text-book of Urology, page 732.

"1. Mentally assured, nervously quiet patient, with a urethra of known patency, preferably 24 F. to 26 F., otherwise adapted to any of the special subcaliber cystoscopes, 18 F., 15 F., or 13 F. 2. Asepsis and antiseptics of instruments, accessories and dressings, patient, operator and assistants. 3. Medium of vesical distention, transparent, translucent, clean, clear and unirritating. 4. Good steady illumination, not so intense as to tire the eye of the operator, or so potent as to burn out the lamp. 5. Bladder as free as reasonable from points discharging blood or pus. Frequently such a bladder is obtainable only after the due administration of local or general anesthetics and gentle but free irrigation with styptics and pus-solvents."

The writer likes to have a mental concept of the following features of the mucus membrane: color, vessels, gloss, continuity, oedema, elasticity, new growth, ulcers, and foreign matters, and finally the contents such as urine, blood, mucus and pus. Each of the anatomical subdivisions of the bladder must be reviewed with this thoroughness as indicated in another contribution.²

² V. C. Pedersen "Topography of the Bladder, with special Reference to Cystoscopy." N. Y. Med. J., Aug. 23, 1913.

The bladder is divided into five regions on its anatomical arrangement. In the original paper I described them as follows:

"First, the posterior lower quadrant, or better, the uretero-

trigonal quadrant, containing the right ureter and its fold, the interureteric fold, the left ureter and its fold, the trigonum, and the posterior half of the neck. Second, the posterior upper quadrant, or by choice the subperitoneal quadrant, which lies beyond the ureteric and interureteric folds, and is not infrequently called the deep base or deep fundus of the bladder. Third, the anterior upper quadrant, or preferably the urachal quadrant, inasmuch as this contains the true apex of the bladder with the remnants of the implantation of the fetal structure, the urachus. This quadrant might also be well known as the apical zone. Fourth, the anterior lower quadrant, which might suitably be noted as the retropubic, inasmuch as it lies immediately behind the symphysis pubis and contains the anterior half of the neck of the bladder. The importance of the neck of the bladder, and the fact that it is best explored with the retrovision telescope, or with the urethroscope, might be regarded by many as reasons for making it a fifth subdivision for office records and the like."

Only in this step by step point by point method may proper distinction and interpretation be reached as to those conditions of which the foregoing elements are only single symptoms, or signs: inflammations, infections ulceration, benign and malignant neoplasms, lithiasis, foreign bodies and the like.

In the therapeutics of the bladder the minutiae are so numerous that they will have to be discussed in a subsequent paper. It is sufficient to say here that the thorough cystoscopist has at least one representative of all the standard cystoscopes urethroscopes, and accessories. The general practitioner who finds in an office one cystoscope and one urethroscope should conclude that he is in the presence of a novice or one who is perfectly willing to make his patients carry the inconvenience of deficient armamentarium. For example, in a certain clinic I have noticed the presence of one cystoscope and no urethroscope. The question arises, what is done as to cystoscopy of prostatics who compel the use of a subcaliber instrument or as to urethroscopy of patients in whom a cystoscopy is not conclusive. As a matter of loyalty to the patients of my clinic at St. Mark's Hospital, I personally take from my office a standard exploring and double-catheterizing cystoscope, operation cystoscope, subcaliber cystoscope, lateral-view and open-end McCarthy cystourethroscopes, and the Buerger cystourethroscope. On the standard and ideal of these instruments no patient is ever neglected.

At a meeting of the Section on Urological Surgery at the New York Academy of Medicine, about a year ago, one of the older men remarked that there is a tendency to invade the kidneys before a full cystoscopy has been made. The writer agrees with this statement without hesitation or equivocation, especially among the younger men. And the practitioner who notes a cystoscopist who slurs the bladder and jumps to the kidneys notes a man of inexperience and careless method. Very often indeed I prefer to do the cystoscopy at one sitting and the kidney exploration at a second sitting. The chief advantage of this method is that doubtful features of the bladder should be seen twice in the same relations and conditions before settling the conclusion that they are pathologic entities. Such a statement needs no argument. It is an axiom of cystoscopy. The features of the ureters which should be detected are position, number, condition, patency, form and stenosis, and the renal conditions always under consideration are nephritis, pyuria, hematuria, hydronephrosis, pyonephrosis, lithiasis, neoplasm, neurosis, displacement and malformation.

Blood in the urine during examination is frequently unavoidable, and means but little if not in large quantities and originating from the mucosa of the ureter

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Treatment of Pain in General Infectious Diseases

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In the early days of medicine the treatment of symptoms largely controlled the attitude of the physician. Modern research has established the fact that symptoms, such as fever, headache, etc., are only expressions of a definite pathologic complex which must be treated from the point of view of the original cause in order to remove the symptoms resulting from it. Thus the empiric procedures of old have been relegated to the background and the importance of symptoms has decreased as the importance of the cause of disorder has increased. The question arises: "to what extent is symptomatic treatment of pain necessary, and from what point of view should it be applied?"

Pain is a sign of many differing abnormal conditions. Primary inflammation of the subcutaneous connective tissues causes pain because the infiltration of the tissues with serum, cells and blood corpuscles brings about tension of the involved areas. These processes are necessary because the accumulation of cells, which constitutes the inflammation, is the first sign of tissue repair; pus formation is accelerated and surgical treatment becomes possible.

This is true in carbuncle, in inflammation of connective tissue and in every case of abscess. Pus formation is necessary. Pain is a signal which demands aid for the patient and, at times, pain itself necessitates treatment. If, for example, the customary method of cold applications by ice packs is used, pain is alleviated, but through such procedure the necessary flow of blood and of cell elements is often checked, and the speedy formation of pus ceases so that the original cause of pain—infection—is not combatted. Hence, through the use of such an aid, an extension of the exciting cause of suppuration and a prolongation of the infection itself is caused; in a word, the symptom of pain comes into the foreground at the expense of the recognition of its original cause. The example of inflammation of connective tissue is only one of many. It is apparent that the problem of the treatment of pain is not to be readily solved. It is therefore necessary that new principles of treatment of pain be substituted for the old. Since we should never disregard the treatment of pain, we must coordinate it with treatment of the cause, so that the natural course of the disturbance of the treatment will be in accord.

There are direct and indirect methods of treating pain: the direct way, by the application of outward agents of the physical or chemical type, to which group local anesthesia may be assigned; and the method of treatment through the central nervous system. Both may give the patient relief from pain without necessarily infringing upon the normal progress of the disease. It is, however, conceivable that these methods may interfere with the resolution of the infection if the dosage be excessive. This is easily understood and should be kept in mind in the treatment of individual cases.

Pain is of especial importance in infections. It appears as the principal subjective symptom in most severe infections and accordingly requires the prompt attention of the physician.

Among the acute infections in which the problem of treating pain plays a part, pneumonia occupies a

prominent place. Two types of pain are noted: the pains in the beginning of the disease, which are localized in the long muscles of the back and often extend to the legs, sometimes affecting the whole body, are recognized as prodromal symptoms common to general infectious diseases. Such pains need no specific treatment; if the patient lies in bed, and the muscles are relaxed, they will subside. Other characteristic pains in pneumonia are those engendered by coughing, localized in the trachea, and pains in the sternal region. There are those who consider the cough in pneumonia as a necessary reaction of the body in order to remove mucus and pus from the lungs. The modern view of the pathologic processes in pneumonia contradicts this conclusion since it is contended that the leucocyte stream which immigrates into the inflamed lungs in the first five days, filling the alveoli, cannot be removed by an act of coughing. Moreover, coughing is a futile strain which draws upon the patient's strength and places an unnecessary burden upon the heart.

The sternal pain and the pain in different parts of the chest are caused by a fibrinous inflammation of the pleura, which is generally found in connection with pneumonia of the involved lobes of the lungs. One or both layers of the pleura are thus affected, and severe pains come as a result of sudden movements attendant upon breathing or coughing. It is acknowledged that the essential treatment of pneumonia rests upon the conservation of the heart's action. Whether the heart should be treated with the early administration of digitalis in order to prevent danger of its failure at the time of crisis, or whether later, when the heart weakens, agents of the strophanthus group should be employed, is not a subject for discussion in this paper. Doubtless the following is true: every muscular effort, every unnecessary cough, every excitation of the patient, creates needless heart strain which later, when the difficult circulation in the lungs demands excess work from the heart, affects the powers of this latter organ. Thus, insofar as it is necessary to spare the heart from strain, treatment of pain to a certain degree constitutes a helpful influence on the heart. The whole clinical picture of patients with pneumonia infection, the essentially heightened color of the face, cyanosis, frequent respiration, cough and sputum (frequently up to 50 cc. a day), and the general restless condition of the patient, would be entirely changed if a consistent treatment of pain were inaugurated, *ab initio*.

Two forms of treatment of pain are to be considered in pneumonia: codein and its substitutes for combatting the local symptoms in the trachea, and general treatment with the specific intention of suppressing pleural pains and of keeping the patient in a quiescent state. In severe cases, morphin is used, in light cases paramido-phenol and pyrazolon-derivates are employed. This treatment should be begun in the early stages of the disease. If morphin is necessary, it is to be given subcutaneously three times a day, in doses of 0.01 gram. If it is not necessary to give morphin, pyramidon (0.2 or 0.3 grams), or some other like agent is given two to four times a day. The dosages given apply to adults. Here also one

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Modern Pyelography

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History

Modern urology was undoubtedly born with the development of the cystoscope by Nitze and its improvement by his co-workers and students.¹ The development of ureteral catheterization and renal functional tests have also been of great value, but of no less importance has been the discovery of the possibilities of pyelography and its rapid development.

Pyelography, like most other discoveries, is somewhat surrounded by confusion as to its early history. Many outline pictures had been taken of the ureters and kidney pelvis, prior to the advent of pyelography, by the use of wire stylets introduced within the ureteral catheter. One of the first attempts in this line was that by Prof. Geza Jlyes of the Royal Hungarian University at Budapest, in 1902.²

Historians tell use that Klose in 1904, was the first, to directly inject the pelvis of the kidney and ureter for the purpose of taking a pyelogram in a case of double ureter.³ The original work of Voelker and Lichtenberg, probably antedates this, although the complete report did not appear until 1906.⁴ A review of this paper shows that the authors were endeavoring to outline the bladder evidently for the purpose of taking what we now call a cystogram. They introduced a five per cent solution of collargol into the bladder and by so doing it was found that some of the collargol worked its way up into the ureters and produced shadows on the plate. This led to further studies along this line and they were soon able to present a series of cases. This really represents the laying of the corner stone of pyelography.

One of the earliest to recognize the wide field opened up by pyelography was Braasch and from 1910 on, numerous contributions began to appear, from him and his students.⁵ I believe he was the first to realize the shadow producing effects of the halogen compounds, which still remain the basis of our modern work. After numerous contributions to the literature, his book on pyelography appeared in 1915 and is still a standard.⁶ In his department at the Mayo Clinic after considerable research they adopted the use of sodium bromide in 25 per cent solution. Up to this time most of the work was being done with ten per cent collargol, twice the strength originally recommended by Voelker and Lichtenberg.

In France the first important contribution seems to have been that of Michael Arana in 1915.⁷ Among the most important contributions on the use of shadow producing substances appearing after those previously mentioned, are, that of E. L. Young of Boston, on the use of mucilaginous solutions of argente in 1915,⁸ and the use of thorium by Burns in 1917 and 1918.⁹

In the year 1919 we were introduced to the use of the iodides of potassium and sodium, the former more particularly being advocated by Prof. Hans Rubritius of Vienna¹⁰ and the latter stressed by Cameron.¹¹ A considerable amount of work was also carried on as to the relative value of x-ray tubes in connection with this subject. Some believed gas

tubes gave the best picture, others pinning their faith to the Coolidge type. The latter is so simple and readily adaptable for all purposes that it is now generally used.

Solutions

Klose used bismuth, but it soon developed that there were fragments of the metal left behind which often produced irritation. No less important was the possibility of arsenical contamination of the bismuth. Collargol was said to readily cause damage of the kidney pelvis, but it must be remembered that this was used in the early days and the possibilities of faulty technique plays no small part.

In view of the large amount of research which work in this line has stimulated, it is not surprising that a number of drugs have been introduced, but it is notable that we cling closely to the halogen group. It is quite essential that we use a solution which will readily drain off and this eliminates such substances as the bismuth solutions, argente, argyror, etc.

Cameron recommends sodium iodide 25 per cent, Woodruff,¹² bismuth and ammonium citrate 10 per cent. Josephs¹³ advises umbrenal which is really an acid free lithium iodide. Lowsley and Müller¹⁴ after extensive experimentation found that sodium iodide gave the best shadows, is non-tonic and easily prepared. As a result of their researches this drug has gained considerable favor.

Preparations of Fluid for Injection

Sterile ampules of sodium iodide 20 per cent and a number of other preparations are now put out by the pharmaceutical houses, and is the best way to handle these chemicals.

Preparation of the Patient

In the urological clinics of this country we are most insistent as to the proper preparation of the patient for roentgen pictures of the urinary tract. In some of our insitutions the patient is placed on a light diet, mostly liquid for twenty-four hours previous to the picture. At other clinics the patient is told to eat lightly and is given an ounce of castor oil the night before and a rather thorough cleansing anema the morning the pyelogram is taken. Oleum ricini is undoubtedly the best medium for this purpose as its action as a rule is prompt and it leaves no possible residue that might cast a shadow. I have however often used salines where a quick action was necessary and have had no difficulties.

In the European clinics, while it is the rule to use cathartics, notably oleum ricini, they by no means regard it as necessary as we do. I once heard such a well known authority as Prof. Josephs say that he had taken many pyelograms, without the use of laxatives, as the well filled kidney pelvis casts such a shadow, that the significance of the picture can scarcely be disturbed by the intestinal contents. Only where a chronic constipation or a well marked enteroptosis or nephroptosis is present or where hard masses may darken the picture, does Josephs regard intestinal evacuation as really essential. Where necessary, he uses fairly energetic drugs, but as a rule the European believes good roentgen and urological technique is attainable without them.



Fig. 1.—Stricture of upper ureter. Shows stricture clearly, with beginning dilatation of the Pelvis and calices.

Fig. 2.—Hydronephrosis with well marked dilatation of the Pelvis and calices.

Fig. 3.—Tuberculosis of kidney and ureter showing typical moth eaten appearance.

Pyelography Table

For the proper taking of kidney pictures it is necessary that we have a proper table. Most of those in use in our hospitals are not well adapted for this class of work, as they cannot be quickly adjusted to the different postures called for in the various urographic techniques.

One of the most simple and readily adaptable is that devised by Lowsley, the essential features of which are as follows: frame of steel tubing not too heavy, legs spread to make it firmer and stronger. The legs at foot of table have rubber feet, those at the top rubber tired wheels, which render it easily movable and yet firm when placed. The Monel metal top is non corrosive and is long enough for a very tall man to rest his head upon it, while a spout at the foot allows fluids to escape into a reservoir. A control wheel at the front is handy to the operator to change the elevation of the pelvis. Bierhoff knee rests of an improved type are attached and cannot rotate, but may be adjusted to any position desired by means of a spring catch. No X-ray tube or holder is attached, and in my experience I find this is more to be desired. The plates are laid upon the top of the table under the patient's body, this retains them where they are most desired and the flat top of the table prevents them from being cracked. The picture represents its use in the semi-sitting posture, suggested by Fowler of Denver,¹ and frequently made use of in pyelography. This table while primarily a cystoscopic adjunct, is quickly adapted to the closely related urographic techniques. The X-ray tube mounted on a movable standard can be quickly shifted from one cystoscopic booth to another by its overhead electrical connections, instead of having several patients waiting for one table, where the tube is attached. This is an important factor in a busy clinic.

Technique of Pyelography

From recent observations made in all of the large urological clinics of America and many of those of Europe, it appears as if investigators were about equally divided as to the employment of the gravity or the injection method. I note still more recently



Fig. 4.—Polycystic kidney. Note the extent of the mass, as is usually the case, the condition of the opposite kidney is the same.

that several clinics are being converted to the injection method. In regard to the size of the kidney pelvis one sees all kinds of quotations and figures. Last summer I heard a prominent European urologist say that the kidney pelvis was smaller than most of us thought and that over five or six cc. was unusual. Most observers give us a little more room often up to ten cc. In cases of doubt it is a very easy matter to find out, by the simple injection of boric acid solution. Incidentally it is always best to use boric acid solutions in the bladder and kidney pelvis as it reduces the possibility of the contaminations that may occur in sterile water.

If one elects the gravity technique, the solution should be placed in a small burette-like glass, the tubing from same being connected with the ureter catheter, and the tube then held only slightly higher than the patient's body. This is a very slow method but a very safe one.

With the injection method a ten cc. Luer syringe is usually used, the fluid drawn up from the ampule and then injected very slowly until the barrel is empty or signs of annoyance appear. The injection should be stopped at once on the appearance of any sign of discomfort, as this is often the warning signal that the limits of safety have been reached. The catheter is usually passed as far as the upper calyx, but necessity may modify this somewhat.

In connection with technique there are several important points aside from the direct injection, that one should give careful consideration:

First—Pyelography is distinctly a hospital procedure and should be performed there.

Second—The patient should lie on his back with the head low, and any fear or anxiety carefully allayed.

Third—Everything should be in readiness and in position before the injection is started, that is tubes, plates, etc.

Fourth—The catheter used should not be large enough to fill the entire lumen of the ureter, a number five F will usually suffice. This allows any excess fluid to drain off from the renal pelvis, back into the bladder.

Fifth—Graduated catheters are best as they give one an idea of the location of difficulties should they be encountered.

Sixth—Drain off as much fluid as possible from the pelvis before injecting.

Seventh—Remove as much as possible of the shadow producing substance from the pelvis, after the picture is taken.

Eighth—Last but not least, I believe it advisable never to do both sides at once, that is avoid the so-called double pyelogram. I believe this cannot be too strongly emphasized. More of this later. I have purposely avoided any description of X-ray technique in this article as it is in itself a very big subject and would carry us too far afield.

Reactions

Reactions occur at times, but as a rule are not severe. Slight painful sensations over the renal area which may persist for several hours, and at times renal colic occurs. Nausea and vomiting or chills are not unusual and cases of pyelonephritis have been reported. Aside from an occasional temperature rise of a few degrees lasting for a short period, I have seen no real reactions in the large hospitals with which I have been connected.

Dangers of Pyelography

Are there dangers in the performance of pyelography? Yes, there are both, fanciful and real. It is unfortunate that our early progress in pyelography, as well as cystoscopy was greatly hindered by the statements of prominent surgeons, that there were great dangers in the method and that patients were often made worse by it. These extreme views were held for long periods and in some quarters are really still present. Aside from this undue skepticism, the possibilities of trouble are often present.

Rupture of the renal pelvis has occurred. Necrosis of the kidney has followed injection of the pelvis under high pressure. I have seen two cases of suppression of urine, one for about twenty hours and another for about thirty-six hours, following double pyelograms. Morton has also reported some cases of this kind. I once saw a death follow a double pyelography in which the autopsy showed an aneurysm of the renal artery.

Some clinics have reported large series of cases, notably the Mayo clinic of almost one thousand cases without a serious mishap, but some others have not been quite so fortunate. How shall we guard against these mishaps? I believe that most if not all difficulties can usually be avoided by a careful adherence to the precautionary suggestions noted under the head of technique.

Pyelographic Readings

In a study of our pyelograms, the following points stand out as essential to our consideration:

First—We must study the size and shape of the pelvis, the normal being called somewhat trumpet shaped.

Second—Does the opaque catheter pass into the upper calyx vertically, or is there a curve and how marked is it?

Third—What is the appearance of the uretero calcine line. (The upper ureter, the lower margin of the renal pelvis and the lower calyx are said to form a symmetrical curve amounting to half a circle; this constitutes the so-called uretero-calcine line).

Fourth—The calyces must be studied in detail, as their size, shape and arrangement differ greatly in the various pathological conditions.

Fifth—The expansion of the upper end of the ureter. This is usually gradual and there is nothing to mark the junction with the renal pelvis.

The principal diseases of the kidney in which pyelography is of most value are hydronephrosis, pyonephrosis, tumors, tuberculosis, polycystic kidney and the lesions due to calculi. We shall endeavor to run over these conditions, somewhat briefly.

Hydronephrosis

Hydronephrosis is usually or in fact I might say always due to some partial obstruction of the kidney drainage either continuous or intermittent. The blockage may be anywhere from the anterior urinary meatus, up to the pelvis of the kidney. I remember well a very marked case of pus kidney that undoubtedly

had as the real etiologic factor a moderate anterior urethral stenosis. It is often really surprising to note what a marked hydronephrotic condition may follow a most trivial appearing, and at times intermittent urinary interference. The lesions of hydronephrosis and pyonephrosis we shall consider together. From a pyelographic standpoint we are interested in the mechanical changes that take place in the pelvis of the kidney due to pressure and which account for the marked variations noted in our films.

Briefly they are somewhat as follows: Incident to the gradually increasing pressure of the stagnant urine, we notice that the general pelvis has a taut appearance, and is dilated. The pressure forces the renal calyx into a battle against it and as this continues there is a final paralysis of the musculature usually on one side of the circumference of the calyx entrance. This allows the stagnant urine to penetrate into the urinary tubules of the papillae and the calyx niches. The cupping of the calices then disappears, the neck becomes shored or broadened and in advanced cases they are simply rounded out areas, projecting from the greatly expanded pelvis. The pelvis often becomes more rounded or even square. The upper border is elevated, the lower depressed, pressure finally wiping out most of the landmarks entirely. The line of the ureter and upper calyx are considerably changed. The catheter no longer enters a calyx, but raises up the roof of the pelvis.

Tumors of the Kidney

The essential features of a tumor as demonstrated by a pyelogram are first renal displacement. Why do we not say mass? Because a mass is usually palpable, and if we can feel a definite tumor mass, those of experience do not recommend a pyelogram, as there is too great a possibility of scattering the tumor cells. The hypernephroma of Grawitz or as Wilson calls them mesothelioma, is one of the most frequent forms of kidney tumors, and is highly malignant. The cells of this form of new growth are readily detached and sent on their way into the general circulation through the renal vein. Incidentally Eisendrath has recently recommended the preliminary ligation of the kidney vein, before handling the rest of the renal pedicle in this type of neoplasm.

Second—Filling defects due to encroachment.

Third—Abnormal position of the renal pelvis.

Fourth—Deformities of the uretero pelvic juncture. There are other characteristics but it is on these four alone that we make most of our diagnoses.

It is true that renal displacement does not take place in all cases, but I am convinced of its great value by the observation of many pictures. The displacement is usually downward or outward, often a combination of these two directions. Filling defects are almost always present and are due to the tumor mass causing pressure on the calices, this pressure at times also lengthens them. The renal pelvis may be raised, lowered or pressed well to one side, usually outward. Its direction is markedly altered and its axis far from normal.

The uretero-pelvic juncture is changed, often considerably that is there a decided curving at this point, or an angulation. At times this is so marked that we can almost call it a kink. In renal tumors it is always advisable to take pictures of the lungs and long bones, in order to determine if metastasis is present.

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Roentgen Ray Treatment of Uterine Myomata*

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The modern technique of treating uterine conditions with the Roentgen rays dates back only several years. Much of the literature on the subject published even as late as the year 1921 can now be transferred to the section on historical medicine. This was made possible by the researches of various workers during the last decade. It was found that there was no direct ratio between the amount of rays absorbed by the tissues of the body and the character and amount of biological action produced. Other factors are known to enter into this relation, namely the structure of the cell, its chemical composition, and its biological characteristics. The younger, the more rapidly growing, and the least differentiated cells respond more rapidly to the influence of the rays than do the older, the highly specialized, and the resting cells.

There is no way of measuring directly a dose of x -rays. The quantity of rays that will produce a mild erythema of the skin which erythema wears off leaving a mild pigmentation is called a skin erythema dose. With this as a basis, a dose of x -rays applied at any depth is expressed in percentage of the skin erythema dose. Seitz and Winz have proven experimentally that while the intestinal epithelium is capable of withstanding 135 per cent. of the above mentioned unit and muscle and connective tissue as much as 180 per cent., ovarian tissue is destroyed by only 35 per cent. This variation in susceptibility of the various cells and tissues of the body makes it possible to destroy the functions of a given organ, and occasionally even a single selected function without injury to the surrounding structures.

Roentgen rays are absorbed by the tissues through which they are made to pass. An organ situated at any depth receives only a part of the rays that are applied to the surface, the reason for it being, that a certain quantity is lost by absorption in the superimposed layers of tissue. At the same time the beam of rays is weakened by distance. The latter is due to the fact that the base of a cone of light increases in area fourfold as the distance from apex to base is doubled. The quantity of rays is distributed over an area four times larger in size and the intensity of the rays per unit of space is proportionately diminished. Although with present day knowledge of x -ray technique and the modern powerful apparatus at our disposal it is often possible to apply to the ovary the full lethal dose (35 per cent. of the skin erythema dose) from one surface area, this technique is hardly ever made use of, because it entails a great loss of time.

In uterine myomata the ovary is often dislodged and is situated at a greater distance from the surface and is often placed behind a larger tumor mass also in very fat women, in order to deliver a reaching dose, the skin has to be taxed to the limit, even then it may be insufficient. To compensate for the deficiency we allow the rays to enter from several angles, using a number of skin areas as ports of entry. The rays are made to cross one another in the desired region and the sum total of the crossing rays constitutes the dose. Two such fields are sufficient for each ovary and the whole course can be administered in one sitting or divided so as to be given on two succeeding days. With the old apparatus

which delivers very soft x -rays, only a small fraction of the skin unit reaches the ovary, because most of the rays are absorbed in the superimposed layers, therefore a large number of ports of entry are utilized for cross firing. This necessitates the reduction of the size of the fields of entry which in turn causes a diminution of the intensity of the deep dose. The reason for it is that the addition of intensity from scattered and secondary radiation which is a powerful factor when large fields are selected is here reduced to a minimum. At any rate the proper dose can not be made to reach the ovary and, those that still resort to the old method, must avail themselves of repeated courses of treatment. The latter are stretched over a period of eight to ten months.

The patient receives a larger amount of rays than with the modern technique, because the ovary, if not destroyed, has a tendency to recuperate during the intervals of treatment. Because of the prolonged treatment the interstitial cells are destroyed and the constitutional effect upon the patient is more pronounced. There is also the possibility of causing serious injury to the blood system.

As the modern method of treating uterine conditions with Roentgen rays is comparatively new, much of the available statistics on the subject is based on results from the old technique. It therefore is necessary to warn the reader to inquire into the method employed, before drawing conclusions as to results. As a good illustration of the above statement will serve some statistics collected by Dr. E. Runge and published in his book on Roentgen ray therapy in gynecology.

Out of sixty-three roentgenologists cited, fifty-nine worked with diagnostic apparatus and employed a layer of shoe leather or one-half to five millimeters of aluminum as a filter, whereas only four availed themselves of modern therapy apparatus employing one-half to one millimeter of zinc or copper for the purpose of filtration. It might be added here that the metallic filter is employed in order to sift out the softer components of a cone of rays and that one millimeter of copper absorbs twenty-five times as many rays as does one millimeter of aluminum.

In spite of the fact that the rays from radium are even more penetrating than the Roentgen rays, the result obtained from radium treatment is by far not as good. This is due to the physical law of distance, which has been partly discussed above, namely, that the intensity of radiation varies inversely as the square of the distance from its source. The deeper the location of an organ to be treated, the more will the dose suffer from this cause, unless the source of radiation is placed so far away from the skin that the difference between the distance from the anticathode of the x -ray tube to the skin and from the same point to the region which we desire to influence is so small that it is practically negligible. Assuming the ovary to be situated eight centimeters from the surface, if we place the anticathode of the tube sixty centimeters above the skin area which we have selected as a port of entry of the rays, we have a focus skin distance of sixty centimeters and as the ovary is eight centimeters deep, the distance between focus and ovary is sixty-eight centimeters. The square of the first is 8,600 and the square of the second is 4,624. The first divided

* Read at a meeting of the Bronx Gynecological Society, Jan. 6, 1924.

by the second gives us .80 or a net loss by distance of 20 per cent. This accounts for the loss in the eight centimeters of tissue. The loss of intensity of rays, before they strike the skin, is many times greater. However, the quantity of rays emanating from a high voltage tube is very great, when compared with radium and this loss is easily made up by increasing the time of the exposure. The latter is impossible in the case of radium.

To duplicate the above arrangement would require an enormous quantity, many gram-hours, of the substance which is practically unobtainable. When radium is used in the usual manner, let us say it is introduced into the cavity of the uterus, the weakening by distance is tremendous. On account of the thickness of the filter and other coverings we may assume that the radium itself is placed about five millimeters from the skin or uterine mucosa. Applying here the above simple arithmetical formula we find that at a depth of only five millimeters below the surface, the loss in intensity is fully seventy-five per cent. and at one centimeter, only four per cent. of the original intensity is present. It is needless to say that the intensity of the rays in the region of the ovary is extremely small. One must not confuse this with the introduction of radium needles within the substance of malignant tumors. The action is different because the radium needles are placed at no greater distance than one centimeter from each other. However, this never comes into question in the case of uterine myomata, as that method can only be used in the case of accessible tumors or after laparotomy.

The effect produced upon the ovary from the application of a therapeutic dose of x -rays is shrinkage of the size of the organ and the disappearance of the Graafian as well as the premordial follicles. The greatest effect is exerted upon the nucleus which becomes swollen and loses its staining properties. The chromatin disintegrates into small particles and finally the nucleus falls apart. The rest of the cell follows this process. The action upon the corpus luteum is not so pronounced. The cells of this structure are not undergoing active division. Furthermore, the thecalutein cells are of mesoblastic origin and are less susceptible to the rays than those that originate in the epiblast. The elements which participate in the formation of the corpus are already present long before the ovum is mature. These cells are possibly, even then, endowed with their special function, which is the establishment and, later, the cessation of menstruation. For that reason, if Roentgen ray castration is undertaken in the second half of the intermenstrual period, the succeeding menstruation is not stopped, but may be more profuse than usual. This is due to a well-known fact that if a dose of x -rays is not sufficient to destroy an organ, it will cause its stimulation. The interstitial cells are not destroyed. As they are supposed to be endowed with the function of internal secretion, an explanation is offered why Roentgen castration is not followed by such pronounced constitutional symptoms as is surgical castration. Neither is there the same tendency toward obesity.

The occurrence of myomata has been attributed to a dysfunction of the ovaries. The fact that cystic degeneration of the ovary is frequent in cases of myomatosis seems to speak for it. In the early days of surgery, this led to the treatment of the latter disease by surgical castration. The results were good. The myoma shrinks practically in all cases after castration, whether surgical or x -rays. In one-third of all the cases, the tumor completely disappears. Some observers believe that the effect of the rays upon myomata is entirely due to the destruction of the function of the ovary. This is substantiated by the effect produced by surgical castration also by experiments which seem to prove that muscle cells are unaffected by doses less than 180 per cent. of the skin erythema dose. On the other hand, other observers be-

lieve that the rays exert a direct action upon the tumor. The fact that a myoma may grow after the menopause and then shrink rapidly after x -ray treatment speaks for it. Yet this argument is not conclusive, as it may be claimed that the probable factor in the ovary which causes the formation of myomata is unaffected by menopause, but is destroyed by Roentgen rays.

Apart from myomata, the other non-malignant uterine conditions which are benefited by Roentgen castration are osteomalacea and the metropathias. Of the latter, preclimacteric bleeding and the so-called essential metropathias are especially suitable. However, there must be a certainty of the absence of disease of the endometrium or adnexa. Many cases will require a preliminary curettage to establish this fact. Osteomalacea is due to a disturbance of the calcium metabolism, either from an overactive ovary or a deficiency of its regulating endocrine opponent. Surgical removal of the ovary cures the disease. Roentgen castration, even of a temporary nature, will rapidly stop the process.

In spite of a certain number of contra-indications to the use of x -rays in the treatment of uterine myomata, there is still fully two-thirds of the total number of cases in which this method is preferable to surgical interference. With the proper selection of cases, a favorable prognosis as to result may practically be given to every patient. This is based on personal experience and upon statistics backed by such authorities as Gauss, Varne-cross, Winz, and others.

Roentgen castration is particularly indicated in menorrhagia in patients suffering from heart disease or such constitutional disturbances as diabetes and tuberculosis, in the very obese and in all women near the climacterium. It is the method of choice even in younger women, except in such cases in which the tumor which causes the bleeding, can be removed without hysterectomy with a possible chance for future pregnancies.

This method of treating fibroids should not be attempted in such cases in which the diagnosis is doubtful. There may be fear of a coincident carcinoma of the uterus or of sarcomatous degeneration of the tumor itself. In these cases it is best to operate and follow this procedure with x -ray treatment, if malignancy is discovered. Sarcomatous degeneration of a fibromyoma which has been treated previously with x -rays is almost unknown. It seems that this treatment acts as a prophylactic against the latter disease. It is not advisable to employ the Roentgen rays in cases which are complicated by other pelvic conditions that require or ultimately may require surgical interference. It should not be attempted when there is suppuration, liquefaction, gangrene, or calcification of the tumor. Even cystic, amyloid, or fatty degeneration may cause the treatment to be a failure.

Tumors which are situated in the horns of the uterus, especially when accompanied by constitutional disturbances awaken the suspicion of adenomyoma and should be operated. When the tumor is so large that it reaches the umbilicus or it produces symptoms of pressure or incarceration of the bladder, ureter, or intestines, it is better to operate than to wait for the slow relief obtained from the x -rays. Pedunculated tumors, whether subserous or submucous do not respond so readily to x -ray treatment and are more advantageously treated by surgical removal.

Finally one must be careful not to apply the rays in cases of myomatosis complicated by pregnancy. Injury to the fetus with resulting monstrosities may be the outcome, as fetal structures are very susceptible to the rays. In this connection may be mentioned one of the former objection to Roentgen castration

(Concluded on page 264)

The New Aspect of Cancer

HARRISON TAYLOR CRONK, M.D.
New York

This paper is sixth in a series relevant to protozoic origin of malignancy and its successful medical control. The series appears exclusively in the *MEDICAL TIMES* and copies of previous articles are se-
curable only until remaining files are exhausted.

Nothing in nature is irregular or disorderly. Cancerous growth then, shown by structure to be both irregular and disorderly, would occupy the unique position of violating all natural law if as advocates of run-away cell theory insist, cancer be due not to extrinsic cause but to inherent cell madness.

Reiteration of the fallacy of such hypothesis is in good order. Minds which are not insular must agree that cancer's origin is extrinsic and under clear thinking will reason that since two causative factors (*bacteria*, *protozoa*) might be culprit, and since sixty years of work done on one of them (*bacteria*) has proven negative; ordinary judgment will ascribe causation to the other factor (*protozoa*) upon which practically no investigation has been centered at all.

The paucity of ideas which have arisen in direction of cancer's cure is readily explained by inheritance of opinion. Caught young, and taught theories which the teacher himself had imbibed from his teacher; student opinion becomes fixed in its theory and rarely escapes from it. Further, the exception to the rule, daring to engage in original thought, finds his trial jury apt to be composed of men of such tenacious opposite opinion, descended to them by inherited teaching, as to discourage those very traits of originality from which alone can progress and enlightenment find birth. Teach a young person a definite thought in science or religion and later years of contrary teaching will have little or no effect toward altering fixed ideals.

Consideration of protozoa opens a new field to research, and discovery of causation of many diseases of obscure origin, is bound to follow in the wake of more extended study of the subject.

The protozoon *Trichomonas* is definitely accused of being the causative factor of those types of malignancy we call carcinoma and sarcoma, because among known protozoa which have had some study expended upon them, it possesses distinct characteristics which seemingly alone fit it for the role. Besides being a globulin feeder, its facultative character, favorite residential sites, great motility, habit of wandering and clustering, and other habitual distinctions, deductively mark it the logical criminal.

When in connection with such hypothesis it is an accepted fact that such animal existence can be extirpated through application of certain parasitocides with the same certainty and facility as higher organisms such as pediculi are extirpated; and when under such administration tumorous growths first cease to enlarge and then follow natural healing course of recession, disappearance or encapsulation; it is sensible to infer that the laws of cause and effect are in operation.

Assuming with sound reason that tumor growth is a natural combat-effort to fight a disease intrusion; Nature here using connective tissue cells just as phagocytes play the soldier part in suppurative processes; it ensues that destruction of inciting cause having been accomplished, there occurs the ever active healing processes of which absorption is a part.

In the growing tumor we will ultimately find, when pathologic technique is developed to point equivalent to the present fixation and staining facilities possessed in bacterial knowledge, sufficient presence of protozoic life in tumor cells as to remove all doubt. The pertinent fact before us at present, however, is that on a fixed hypothesis of cause—an internal parasite—application of a parasiticide produces a wished for result.

That this had not occurred in previous therapy applied on principle of similar theorem has been because heretofore no method of application of medication of sufficient strength has been known. Manifestly the goal to be reached could be reached only through all pervading circulatory channels because of the hidden and often obscure sites of the disease and the presence of widely disseminated unlocatable metastases.

All parts of the body are susceptible to cancer in one or another of its forms. Approximately fifty per cent of cancer deaths in total are caused by lesions existent along the course of the digestive tract, male deaths preponderating. Localities distinctive to females are the site of the death lesion in about 50 per cent of female cancer mortality. How much this favoritism of site can be associated with *Trichomonas*' favoritism of habitat in its benign existence—the upper small intestine in both sexes and the vagina in the female—can at present only be conjectured.

The terrible prevalence of cancer is thoroughly brought home by the following illustration. Of four families, each consisting of husband and wife with their respective parents, a total of six individuals per family not counting children, twenty-four individuals in all, *three* are destined to die of cancer unless cure be affected. The ratio is increasing so that soon in such a type-family (still excluding children) the toll will average one death per family. The red plague is more steadily ferocious than was the white plague.

* * * * *

We are still prone to attribute irritative factors as of considerable import in cancer causation, but as we progress in knowledge present opinion will be largely modified. A tendency to magnify irritation's importance appears to be too prevalent. We must sail on an even keel in that consideration just as we must upon diet and nutrition in their relations to the disease. Irritation is a contributing cause and nothing more.

We are safe in standing upon the same firm ground in that relation that we stand upon in our contemplation of all disease susceptibility, and only extremists will depart from that position. Lime deficiency in rachitis, and tuberculosis and other wasting diseases occurs either through faulty nourishment or a faulty metabolism under proper nourishment. It is well established that iodine deficiency existing from similar cause, induces goitrous manifestation, and so on down the list. But generally when we come to diseases of the degenerative years, occurrent after the building and more static periods of life, present knowledge makes us quite safe in attributing

vulnerability to acidosis. I think time will not alter that belief but rather reinforce it.

Yet to assign acidosis as specific cause of cancer is going too far and altogether outside the bounds of intelligent reason. I prefer to charge acidosis with being chief cause of many of the diseases of the degenerative years, but only a contributing cause to cancer by affording a fruitful soil.

The tendency of enthusiasts to impute cancer's cause to diet is founded upon a general ignorance of food in its relation to health. Graduates in medicine of a few years back were taught only a mere smattering of this knowledge and graduates of today get little more. The domain has been preempted by student specialists who, however great their general knowledge may be, are short on medical knowledge just as the doctor is short on food knowledge and the two have got to go together if most intelligent headway is to be made.

In practice with our cancer cases we are confronted with an existing status, the result of piled up years of wrong living. A preponderance of acid elements accumulated through forty-five years or more from eating too much meat, fish, eggs, tea, coffee and cereals in comparison with the base carrying foods, milk, vegetables and fruit; cannot be undone by a few weeks' improved regimen. It is cheering to know, however, that if sufficient time exists to attain result from more intelligent diet, the native elasticity of our wonderful bodies will do its part in yeomanlike fashion, despite the fact that youthful years are far behind.

* * * * *

Once protozoic origin of malignancy becomes accepted fact as will eventually be the case, reformation of idea as to cancer's communicability will transform present notion. We will adhere to opinions taught us by experience; that since physicians, surgeons, nurses and attendants closely in contact with cancer cases do not contract the disease from patients, the disease per se is neither infectious or contagious. We will recognize that two elements are requisite to cancer manifestation; one a specific protozoic germ which abounds in similar frequency to the tubercle bacillus, the other a human vulnerability afforded by general tissue condition which permits lodgement and propagation of these protozoa in a living culture medium, with resulting tumor growth exhibited as Nature's resistive effort.

Accordingly we will treat the patient himself as well as the disease. As best we may, we will endeavor to alter the character of the tissue soil from one favoring cancer growth to one less favorable to it, by transforming through diet and other means, acid conditions frequent to the degenerative years, into conditions of health disposing alkalinity; for acidity is characteristic of death and alkalinity is characteristic of life.

Using tuberculosis for handy comparison our minds while admitting the element of hereditary predisposition in tuberculosis will not admit it in cancer. We will arrive at this conclusion because surviving infants are frequently born deficient in mineral constituents such as calcium and phosphorus, absence of which from the tissues makes more fertile soil for tubercular development; while surviving children are not born with tissues in that condition of acidosis characteristic of the degenerative years, which provides a better culture medium for cancer propagation.

Irritation as a predisposing agent, must be viewed

for what it is and nothing more. It adds to vulnerability when coexistent with susceptible tissue soil and can if persistent and strong enough, be sufficient of itself to prepare the ground for cancer inroad in an otherwise quite normally tissue individual; as with pipe smokers, eye-glass wearers, chimney sweeps, Kangri basket wearers and others. Injury, the so frequent excitant of giant-cell sarcoma, belongs to this category. Has it escaped devotees of the Cohnheim hypothesis that a misplaced bit of embryonal tissue might mechanically become quite as much of an internal irritating factor, predisposing by its presence, as a hot pipe stem or a Kangri basket?

Without proper dual consideration in treatment we will fall short of fullest efficiency and as we successfully treat the person in connection with treatment of the disease there must occur a strengthening of resistance which will help materially to hold the cured patient, cured. Some good men have stubbed their toes by attributing an all-importance to this tissue-soil factor and its correction by improved regimen. They have stumbled because of a combination of unbalanced enthusiasm, with a weak knowledge of foods in their relation to tissue-making, and a non-consideration of possible protozoic cause of the disease.

Tendency to recurrence is a constant trait in the cancer patient which is readily explainable by presence of fertile soil and inciting germ cause even though metastases do not exist when surgery removes a main lesion, surgery here playing the role of an insulting irritant. The need of both a systemic treatment and devotion of care to general physical welfare through modification of soil conditions, is thus again emphasized.

12 Fifth Avenue.

Urinary Examination

(Concluded from page 245)

by friction and not from the kidney substance by penetration of the catheter. Boiling the urine to coagulate the blood and filtering out the coagulum make it possible to use such a bloody specimen for the phenosulphonaphthalein functional test.

The other practical characteristics of urethroscopy and cystoscopy will follow in later articles. Attention must be drawn, however, to the fact that no shadow in relation with the upper urinary organs and passages should be regarded as proof of stone unless the radiographic catheters are in the ureters. Elsewhere^a I have shown that there are 27 different sources of false diagnosis of stone in the kidneys or ureters because shadow catheters were not used in the study of the cases. It therefore follows that except in perfectly simple and patent cases of stone in the upper urinary passages no diagnosis can be accepted as final unless the shadow catheters are in place. This is a very important detail which in a large way the general practitioner and general surgeon must learn and follow.

45 West 9th St.

^a Ibid. Page 923.

A New Medical Journal With A New Purpose

Dr. Harold Swanberg of Quincy, Illinois, has established the *Radiological Review*, which is to be devoted to the progress of x-ray and radium as it relates to the physician and the dentist. It will be entirely practical in its nature, containing articles on roentgen diagnosis and radiation therapy together with abstracts from the best literature on the subject. The initial issue is well printed and contains a wealth of excellent clinical material. Those physicians interested in these subjects will find in the *Review* the very type of journal which they need.

Medico-Legal Reflections of a Psychiatrist*

CARLOS F. MACDONALD, A.M., M.D., LL.D.

New York

(Concluded from October Issue)

Feigned Insanity

It is a popular notion that it is an easy matter to simulate or feign insanity successfully. The fact is that one could scarcely undertake a more difficult role. To succeed in shamming insanity, so as to deceive a skilled observer, one would require not only to be a consummate actor, but to be well versed in the mental and physical symptoms of the different forms of mental disease—and to possess unusual powers of endurance. The average criminal, being entirely ignorant of the symptoms of insanity, usually over-acts his part, and fails to present a consistent clinical picture of any form of that disease. The "symptoms" he presents are usually a medley of symptoms in which he mixes up the various forms of insanity indiscriminately. Furthermore, his "symptoms" usually subside when he believes he is no longer under observation. Consequently, the writer has no hesitancy in saying that it is well-nigh impossible to simulate insanity so as to deceive a trained psychiatrist, provided the latter has sufficient opportunity to observe the case. Sheppard, in his *Lectures on Madness*, referring to the feigner, says:

"Commonly he does not know how difficult and sustained a part he has to play for even a remote chance of success, and the curtain falls upon a grotesque and blundering farce more quickly than he anticipated," and again. "Those who are acquainted with the genuine article will soon discover how miserably he is over-acting his part, tear off the mask and expose the imposture. Bear in mind that cultivated and refined malingerers, such as Shakespeare has depicted the Danish prince, are of very rare occurrence."

While it is true that a "trumped up" defense of insanity is frequently offered in criminal cases in which there appears to be no other avenue of escape from responsibility for their crimes, the fact is that a dishonest plea of insanity very rarely succeeds. During an experience of more than forty years in the observation of such cases, the writer has personally known of but three instances in which a sane criminal escaped conviction on the plea of insanity by means of shamming.

Whether a lawyer is ever justified in defending a client on the ground of insanity when he knows that client to be perfectly sane, is an ethical question which may properly be left to the legal profession.

Should the Plea of Insanity as a Defense to an Indictment for Crime be Abolished?

Somewhat recently a committee of the New York Bar Association, appointed to consider the subject of "The Commitment and Discharge of the Criminal Insane," taking for its text the *Thaw Case*, which it characterized as "a disgraceful farce," said, among other things:

"Has not the time come in the development of our system of penology to relegate to the realm of the obsolete the assumption that an insane man cannot commit a crime? In other words, ought we not to abolish the defense of insanity and leave as the one issue to the petit jury: 'Did the accused do the forbidden deed?'"

The committee then goes on to say that if the accused did not commit the act charged, he is innocent; if he did, he is guilty, and that the jury should have nothing to do with the state of his mind at the time the act was committed; and that, however legally right under the present definition of insanity, as laid down in the Code

of Criminal Procedure, it is wrong, sociologically, to find a man not guilty on the ground of insanity. The committee suggested that its views, if sound, could be put into effect by revising Section 20, of the New York Penal Code, with the following words: Insanity or other mental deficiency shall no longer be a defense against a charge of crime; nor shall it prevent a trial of the accused unless his mental condition is such as to satisfy the court upon its own inquiry that he is unable, by reason thereof, to make proper preparations for his defense.

The committee, while refraining from recommending legislation to effectuate such a radical change in the criminal law, on the ground that public opinion may not yet be ripe for the same, declared that the question ought to be discussed, and requested an expression of opinion from members of its own organization and others interested in such questions, as to whether insane persons should be made amenable to the criminal law. In view of this request, it seemed to the writer that the subject is one which might properly be brought before this Society, for discussion.

It need hardly be said that this suggestion of the committee of the Bar Association, the adoption of which it impliedly advocates, is by no means a new one, it having been advocated, in substance, from time to time, by both alienists and medico-legal jurists, in condemnation of a system which imposes upon a jury of laymen the determination of abstruse questions in medical science.

Without undertaking to set forth all the reasons on which the conclusion that the legislative experiment of abolishing the defense of insanity in jury trials of indictment for crime is based by the advocates referred to, the matter may be briefly summed up in the statement that the determination of the mental condition of a person accused of crime naturally belongs, not to the tribunal, the jury which determines whether as a matter of fact a crime has been committed, but to the tribunal, or rather the successive tribunals who, after a verdict of guilty has been rendered, determine the nature and quantum of the punishment. In other words, that the work of the jury in ascertaining the mental condition of the accused in order to determine the question of crime, should be transferred to the judge and to the pardoning power, i.e., the Governor.

Objections to the change proposed by the committee of the Bar Association will naturally revolve around the proposition that there is no crime without criminal intent. Respecting the question of intent, about which much of a metaphysical character has been written, it must be conceded that in a large number of criminal cases, the jury must ascertain the intent of the accused in order to determine whether or not a crime has been committed. The question of intent in these cases, although intimately connected with the question of the mental condition of the accused, nevertheless is plainly distinguishable from that question. If Harry K. Thaw, when he shot Stanford White to death, supposed he was discharging a bullet into a post, then he had no intent to kill, and could not be found guilty of murder, although he might be guilty of a minor degree of homicide. His real crime would be intentionally discharging

a loaded pistol in a crowded place of amusement with intent to drive a bullet into a post, which act resulted in causing the death of a human being, such result being one which a reasonably prudent man must be presumed to be able to contemplate as highly probable. Now whether the question of sanity or insanity should be left with the jury in certain cases where they are obliged to pass upon the question of criminal intent, is a question which the writer would not undertake to determine. Thaw did not intend to discharge a bullet into a post; he discharged his bullet intentionally into White's body, knowing it was White, whom he intended to kill, and he did precisely what he intended to do. The defense was that Thaw's intention was an insane intention, and consequently he was not guilty of effecting the death of White.

What, let us ask, would be the practical result of abolishing insanity as a defense before the jury against the charge of crime and making it a defense before the judge and before the pardoning tribunal? In answer to this question it may be said that this function naturally belongs to the court and to the governor, or other pardoning power, and is exactly in line with what these officials are doing almost daily. After the jury found the defendant guilty, there is no instance, not even murder in the first degree, where the extent of the penalty is not determined by the court or by the pardoning tribunal.

In the State of New York when a man is convicted of murder in the first degree, it is the duty of the court to impose a sentence of death, and it is the power of the governor to pass upon the question, whether or not the penalty of death, which the court was obliged to inflict, should not be changed to imprisonment for life, or for a term of years, or to a pardon outright. Moreover, in the vast majority of criminal convictions, it is the duty of the court to determine whether sentence should be suspended or whether the criminal should be punished by a fine or by imprisonment. The amount of the fine and the terms of the imprisonment are largely within the discretion of the court. Hence it logically follows that no judge can discharge his function of measuring out the penalty without taking into consideration the mental condition of the convict and the effect which enticement or provocation might have had upon a man whose mind was disordered. It is safe to say that the trial of Thaw would have occupied but a few hours if the question of his mental condition could have been excluded from the consideration of the jury and left to the determination of the court, aided by a commission of competent experts, selected by the court. A few days at most would have sufficed for this last inquiry, and the result would have been precisely what was reached after two farcical trials, with all their disgusting details, which lasted for many weeks at an enormous expense to the family of Thaw and to the State of New York. Had this method of procedure been adopted, Thaw would have been placed precisely where he was placed, namely, in the Matteawan State Hospital for Insane Criminals.

In conclusion the writer would respectfully submit to this Society for its consideration, and, if deemed expedient, for an expression of its opinion thereon, the following question:

Would it not be a more satisfactory way to deal with criminal cases, especially capital ones, in which insanity is pleaded as a defense, to keep the question of insanity entirely out of the case during the trial, and allow the jury to pass only upon the question of the guilt or in-

nocence of the accused, irrespective of his mental condition. Then, if a conviction is had, let the court appoint a commission of competent psychiatrists to determine the defendant's mental condition, and if he is punishable by reason of mental disease or not. If the function of the jury were restricted to a finding on the facts, that is, if the defendant committed the act as charged, and subsequently, the question of his mental condition were determined by a commission of competent experts appointed by the court, the finding of such a commission would be accepted by the public, both lay and medical, and there would be no danger of a miscarriage of justice.

The writer does not pretend to say if this would be feasible, but he believes that it would be preferable to the present method of determining the mental condition of a defendant, while at the same time it would tend to mitigate much of the scandal which too often arises in connection with the plea of insanity as a defense to an indictment for crime.

"When Doctors Disagree"

In connection with this branch of his theme, the writer would take occasion to suggest that it is the duty of the medical profession to raise its voice in solemn protest against the tendency which has lately grown up to heap upon it ridicule and abuse because "doctors disagree," and that consequently all, or substantially all, doctors are dishonest. As a matter of fact, doctors are no more prone to disagree than any other class of individuals, where matters of opinion are involved. On the other hand, lawyers are notorious for their disagreements. In fact, in every case that is tried in court we find the contention of counsel on one side diametrically opposed to that of the other side; and this, too, on substantially the same state of facts. Judges are also noted for their disagreements. The higher courts frequently reverse the court below, clear up to the court of last resort; and it is safe to say if there were still a higher court, it would be found overruling the court of appeals. How frequently, too, we find the body of judges constituting the appellate courts divided in the decisions they render, the issue being decided oftentimes by a bare majority of one. And yet nobody would think of suggesting that these judges are dishonest simply because they happen to disagree.

Mr. William A. Purrington, an eminent member of the New York bar, and one of large experience in medico-legal matters, in a paper referring to the disagreements of judges whom he characterizes as "that great body of experts who rarely if ever go upon the witness stand" says:

"I have sometimes wondered whether those judges who have been most denunciatory of the differing conclusions at which reputable medical experts have arrived upon the facts in evidence, have reflected that they, too, belong to a body of experts in the law, whose members often differ, very honestly and ably, in drawing conclusions, even from agreed facts."

An editorial in a New York newspaper calls attention to the statement of a lawyer that of seventy-five decisions rendered on a certain day by the Appellate Division of the Supreme Court of this State, only thirty-seven were in affirmance of decisions rendered at special or trial term. The remainder were either reversals or modifications, nearly all being reversals. A recent writer has jocosely suggested that "medical expert disagreement is a harmonic symphony compared to judicial disagreements," not to speak of the frequent disagreements of lawyers and others. All of which goes to show that no judge, no lawyer, or no expert in sacrosanct.

15 E. 48th Street.

A Brief Review of Some Cases, in Their Medico-Legal Relations *

DAVID EDWARD HOAG, M.D.,

New York

I am presenting a few cases that have come under my observation within the past few years, wherein it was my privilege to act as advisor. I have tried to observe in these cases, what is regarded as the judicial attitude, having in mind as I think we all do, or should do at least, the prevention of any course of action that would prove a detriment to the individual, and not be for the best interests of all concerned.

In cases presenting a medico-legal aspect, we should sedulously leave no stone unturned, to keep our cases from the ordeal of court procedure. One should keep it paramount in mind, that the chief province of the physician in cases of negligence, or where a suit for damages is in mind, is to convince the patient, if it be within the limits of reasonable certainty, that the alleged injury is not great enough to appeal to the Courts for adjustment. It is better I am sure to err on the side of the defendant than on that of the plaintiff. The same rule applies in regard to those upon whom we are called to pass an opinion, among the mentally afflicted.

Although it is our duty to the individual, their families, and the community at large, to protect them from danger—still it is not meant that the offices of our examiners should be used as a short cut to get rid of an unruly member of society. Our asylums and private retreats are filled to overflowing now, without adding to the burden.

It is not so long ago that there appeared in the public prints an article in which the writer, who had been in conflict with the lunacy laws and the Courts, was attempting to present the evidence of a so-called lunacy trust. It is not intended to minimize the need of careful investigation of all cases of alleged injury seeking adjustment, for many of them are honest and only in search of justice. It is not meant to convey the hint that we should not subject to careful scrutiny all cases of alleged mental defect, for the general welfare of the individual, and of the community at large, must not be lost sight of; but we should strenuously resist the use of our offices in aiding or abetting fraud or deceit.

Case I. This case was referred to me by Dr. G. who at that time was in the military service, and as he was about to return to camp at the time he was notified about the case, we did not see the patient together. One gains an insight to many cases, a sort of psychological conception of what confronts him, by the happenings leading up to the first visit. This case illustrates in a sense what I have stated.

I received an early morning call on the telephone—an excited demand to come at once; that the person was in great danger, and that there was need for haste and secrecy. Upon my informing the lady that I could not come for twelve hours, she replied, "All right, come then." Arriving at a beautifully furnished apartment at 7 P. M. I was met by a wildly excited French woman, who began gesticulating to me and talking in French. Finding I did not readily understand she called her brother who had been guarding the patient, whom I had not yet seen. The story was that the patient—a young man of twenty-five who was the paramour of the French woman—had tried to murder her the night before. She had waked up finding herself gripped by the throat, but finally managing to get out of the grasp exhausted and breathless, had summoned

aid. She had black and blue marks on her throat, as proof of her statement. She stated that he had been moody, depressed and irrational all day, that is, the day following the assault, and denied all remembrance of attacking her.

She stated that she had known him several months, that prior to this he had been of a cheerful happy disposition; used to play and sing a great deal; that he was a professional concert pianist; and that his parents were wealthy. She declared that he had suddenly gone insane and that although they had loved each other dearly, despite the disparity of their ages, she being forty-nine, that now she was afraid of him and demanded that he be at once sent to the asylum. I then went into an adjoining room to interview the patient, much against the protests of the French woman and her brother, who declared it might be unsafe for me to be alone with him. I found a very good looking young man athletically built, slightly erratic in manner, but apparently highly educated and intelligent. He held himself in what I believed to be pretended reserve at first, but as soon as he found that I was approaching him in a friendly and interested manner, he became less reticent. He stated that he had served two years in the aviation forces; had been honorably discharged from the service, and that he had fallen from his aeroplane several feet, striking on his head, rendering him temporarily unconscious, and that he had had headaches ever since. He declared that he knew nothing of the attack of which he was accused, that he loved the woman dearly, and that it was she who was crazy.

Having formed a tentative opinion of his case, I asked him to accompany me to a hospital, which he was willing to do. I told him to get together his belongings and to be ready in half an hour, and summoned a taxicab. When I told the French woman of my plans she made a great protest, that he must not go to the hospital but to the asylum, and that she would at once call the police to take him there. I informed her that the moment she reached for the telephone to summon the police I would also reach for the phone and telephone the Police Department, to restrain her from unlawfully attempting to deprive the man of his liberty. The ruse was highly successful. I got away with my man in the cab. I told the driver to go anywhere. I told my patient we were on the way to the hospital. He desired to know what kind of a hospital, and I told him the ordinary kind, and that on account of his alleged injury and symptoms I would have him x-rayed, blood tested, eye grounds examined, and spinal puncture made, to all of which he demurred greatly, saying he did not need it, and finally flatly refused to go to any hospital. He finally compromised and agreed to go to a hotel for the night and have the tests performed the following day.

You will already note that I was merely sparring for time, and that I had already made my diagnosis, not being that of insanity, but of feigned insanity. Here again it was but necessary to establish a motive, which I already had in mind. I left him at the hotel, under the care of a male nurse, and went back to the apartment of the excitable French woman. I told her she need have no more fear, and then confronted her with the accusation, that realizing that their illicit relations must eventually cease, she had concocted the story of the murderous assault, showing her motive to be that of being able to say to her friends that he had gone insane and that she had to be rid of him, which to a woman of her temperament would not be nearly so humiliating, as that he had grown tired of her. To my patient the next day (I was looking for a confession by this time) I tendered my congratulations on so successfully breaking away from an undesirable liaison—all of which he admitted, and finally confessed that his action was a sham and a pretense, including his effort to mildly choke her. In other words he made her fear him, which was the only way his weakened and unstable manhood could find to be rid of her. I kept him under observation for some time after that, and finally induced him to go back to his home in a distant city, with a warning as to his narrow escape from being railroaded to Bellevue. According to last accounts he is leading a normal life.

Be on the alert for a feigned insanity—the motive not being hard to discover, especially if there is a woman in the case.

Case II. Referred by Dr. S. An unusually interesting case. The patient was a boy of twelve years, and gave a history of being struck on the back of the head by a stone thrown by a

* Presented before the Society of Medical Jurisprudence, May 12th, 1924, at the New York Academy of Medicine.

park employee. It was not intended to strike the boy, but merely to frighten him. The boy fell to the ground and was unconscious for a few moments only. He bled freely from a scalp wound, which was dressed by an ambulance surgeon and the boy sent home. He remained up and about during the remainder of the day, and for two days afterward. On the following day which was three days after the injury the boy was restless, feverish and fatigued and was put to bed. On the following morning he was completely paralyzed in the lower extremities and in one arm.

It was shortly after this that Dr. S. was called to the case. In summoning me he said, that he felt that he had a brain injury to deal with, perhaps a fracture of the skull, or at least pressure from hemorrhage. Finding the boy perfectly bright, no stupor, no fever, and with a flaccid atonic paralysis and cold extremities, and reflexes entirely gone, I made a diagnosis of anterior poliomyelitis of the usual onset, and having nothing to do with the injury, but merely co-incident thereto. The diagnosis was accepted by the family physician, but rather reluctantly by the family. That the boy might have appropriate treatment, and for other reasons, I suggested that he be sent to Willard Parker Hospital, where the diagnosis was confirmed by examination of the spinal fluid, and again corroborated at the Neurological Institute where he was transferred later. A short time afterward a lawyer got in touch with me, saying that a case could easily be made against the city, inasmuch as the boy was originally injured by a city employee. I explained to him where he was in error, and nothing further developed. I saw the boy six months later, much improved, but with the residual type of paralysis characteristic of anterior poliomyelitis.

Case III. I was called by the management of a high class apartment hotel on the West Side, to give an opinion on the mental status of one of their guests. She had taken up her residence at the hotel a few weeks previous to my visit, accompanied by several dogs, a few trunks, plenty of diamonds, and other so-called appurtenances of wealth. Her eccentricities worried the management to some extent. She kept to her room constantly and had her meals served there. She soon engaged a trained nurse and summoned two or three different doctors to treat her. She was continually telephoning friends day and night, that she was worried and ill, could not sleep, etc.

In spite of all this she was giving a great many dinner parties in her room. The night I went there one of these dinner parties was in progress. I neglected to say that one of the principal worries of the management was the fact, that they could get nothing in the way of money from this fair patron. On going to her room the night in question I was escorted into the room by the nurse and elaborately introduced to a goodly company, consisting of, as nearly as I can recall, two lawyers, two doctors, and a few stage celebrities. I was asked to partake of the repast, which I respectfully declined to do. I recognized an old acquaintance in the woman I had been asked to see, although she did not remember me, which may have been subterfuge on her part. Two or three years before, this woman had sued her husband for divorce, and in her claim for alimony, stated that she was the victim of general paresis, for which her husband had been responsible. I had testified in court at that time not only, that she did not have general paresis, but even if it could be proved that she did have it, her husband could not have been responsible, for they had lived together but a short time.

Nearly at the close of the repast, this woman suddenly threw up her hands, said she was in great pain, and believed that she had been poisoned by some of the food or drink. The nurse and one of the so-called lawyers said they felt similar symptoms. The dinner party broke up in pandemonium, the management and the chef having been sent for, and an investigation was promised. I made a hasty exit, and reported to the hotel management the following day, that I considered their guest a professional blackmailer and a dangerous woman. She later proved to be a hotel deadbeat.

This particular hotel kept her for a week longer, trying to get their bill paid, but in vain, and finally seized her trunks and ordered her to leave. I understand she went from there to the Gotham, from which she was again ordered to leave. She next turned up living in a furnished apartment on Riverside Drive, conducting a school for interpretative dancing. She came to my office about this time, very penitent, saying that she had been unfortunate in certain speculations, but that things looked rosier, and that she would soon pay up all her indebtedness. She then told me, that she had been placed in the hotel from which she had been at first ousted, by the vice-president of a large corporation in New York, naming the corporation as well as giving the vice-president's name, with whom she claimed to be on very intimate terms. She stated that on the day prior to going to the hotel she had visited him at his office

in the building owned by the corporation, and that while descending in the elevator, and while making her exit from the same had fallen, striking her head against the iron gate—all of which was due to the carelessness of the elevator operator. She further stated that the vice-president had agreed to look after her, paying her expenses, etc., and that he had not done so. She believed that her head had been injured, and that she was going to sue him, and asked me to conduct the medical side of the case. I regarded her as a colossal fraud, and informed the vice-president of my suspicion, and offered then and there to be his witness, if anybody's. The matter died down, some settlement being made.

She then drifted out of my sight until the following year, when she sprang up as a war worker, and sought money successfully for many pretended charities. She was arrested, tried, and convicted of fraud, and is now serving time in Auburn. There is no comment in this case except, that of the proverbial gullibility of New York's native sons. No more verdant field for gold brick speculators exists, far outvaluing the fertile valleys and sloping hillsides of neighboring States.

Case IV. This case was one in which a woman of rather large estate became mentally unbalanced, and in which I was called in consultation by the family physician. The patient was in a state of profound depression bordering on acute melancholia, and I recommended that she be sent to an institution. The family were opposed to commitment, and so she became a voluntary patient at a private sanitarium. About two weeks later a member of the family requested me to accompany her on a visit to the patient. She informed me that a lawyer chosen by the family for his fighting face, as she expressed it, was to accompany us. My suspicions were aroused because of the fact that the attorney who had represented the estate for a number of years had been superseded.

It transpired that the object of the visit was to have the mother sign a power of attorney, giving a member of the family power over the estate. I objected to this procedure on the ground that the patient was mentally incompetent to sign any legal document. However, the proprietor of the sanitarium felt that he had no right or power to interfere, as the patient was not one duly committed. I informed both the attorney and the member of the family that they were engaging upon an irregular procedure. Although the patient strenuously objected, she was finally coerced into signing the power of attorney. The attending physician, the nurse and myself were asked to be witnesses but refused. The notary public who acknowledged the signature, inserted under his name "signed under protest." The following day the Trust Company, having the estate in hand asked my opinion, as to the mental competency of the patient, to sign such a document. As is self evident my opinion was a negative one. She was soon removed from this sanitarium to another, was ordered to be removed therefrom in two days' time, due to the tactics of the family in trying to force another power of attorney. This woman has been dragged from one sanitarium to another, the family steadfastly refusing to sign a petition for commitment, although repeatedly advised to do so.

At one period the family demanded that another psychiatrist be called in consultation, which was gladly welcomed. His opinion and diagnosis concurred absolutely with my own. This defenceless old woman, docile, easily managed, never greatly disturbed or violent, has been abused in many ways. She has been forced into signing orders for the payment of extravagant bills, as well as leases on property, in reality beyond the means of the estate to maintain. In summing up, all this confusion, all this annoyance, to this poor unfortunate woman, might have been avoided if this family could have been convinced that commitment was the best thing to do. Or, if there could have been some legal way, either to compel them to commit, or to insist on the appointment of a committee of the estate and of the person.

Case V. Referred by Dr. L. A young man, thirty years of age, a musician of more or less prominence, a teacher of music and piano accompanist. His ancestry was unknown, he having been a foundling. Even his foster parents were dead. He lived in the same apartment house, but not in the same apartment, of the young lady to whom he was engaged and who at the same time stood sponsor for his care and treatment. I found the patient bearing a syndrome very closely resembling dementia precox, with the character of delusions and hallucinations of the paranoid type of that disease of that disease. My advice was that this man be removed to a place where a person of his particular mental condition could be kept under observation; for two other reasons, that he had no home; and that he must be taken away from the society of the young lady to whom he was engaged.

In looking for motive I read in his case that this was what he desired. The dread of the responsibility of matrimony,

(Continued on page 264)

Toxic Effect of Local Anesthesia (Continued from page 243)

B. Cocain

3. Alarming symptoms occasioning great anxiety followed after packing the nose (both cavities) with 10% solution of cocain.

C. Butyn

4. I use 8 c.c. of a 2% solution of Butyn for injection in tonsillectomies; out of 300 cases there were four cases of severe convulsions followed by profound shock.

(The manufacturers of Butyn state distinctly that one per cent only of that drug should be used for injection.)

(One is compelled to conclude that the writer has a special Providence watching over him. E. M.)

5. I injected 12 c.c. of a 2% solution of Butyn for tonsillectomy. Violent convulsions and coma followed. The patient recovered! These last cases bear out the contention of the Committee that the tendency in the profession is toward excessive doses of these drugs.

6. We have been using Butyn in our local tonsil work for the past two years and have never noticed any toxic effects from the use in the strengths in which we use it. Our practice is to paint the throat—the pillars—with 10% Butyn twice at intervals of 5 minutes and then just before operating inject about the tonsils with 1/10 of 1% Butyn solution with 1 drop of adrenalin to each dram of the Butyn solution used, in all, about 6 drams of the solution for the two tonsils.

We have not tried Butyn in the eye. We have tried it in the nose for submucous work, using 5% solution with adrenalin the first packing, repacking at the end of 20 minutes with 5% Butyn without the adrenalin. We find that when used in the nose, the anesthesia is wonderful, but the post-operative edema is so severe and lasts so long that we promptly discontinued the use of Butyn for intranasal work.

Further Fatalities Following Local Anesthesia

Six additional fatalities following the use of local anesthetics have come to the notice of the chairman of the permanent committee. These came too late for inclusion in the report of March 15th. None has come since publication. One of these was published in the *Laryngoscope* (November 15, 1923) the five remaining were reported voluntarily to the Committee.

Two additional ones were received but were manifestly not due to the drug in question and they were excluded from the list. One followed an injection of Novocain for tooth extraction. Death in the chair without convulsions. Autopsy showed grave valvular lesions of the heart.

The other was alleged to be due to Butyn. Death occurred four days after an operation with all the symptoms of septic infection. Aside from the sepsis, we feel that death must follow promptly with all the symptoms, to be acceptable as a death from poisoning. These two were rejected.

The remaining six were from:

Procain	1
Cocain	1
Cocain-Procain ..	4

Two were for nose operations and four tonsil operations. Four were males, two females.

Cocain-Procain Death

As in our former report, physicians state that death was due to procain. Careful investigation shows in

nearly every case that cocain was used with it, and hence both drugs must be mentioned as causal factors in the poisoning.

Here is a typical case:

Female, age 17, came to clinic for tonsillectomy. Patient sitting. Throat was swabbed a few times with a 10 per cent solution of cocain (amount not stated). About 2 c.c. of a 1 per cent solution of Novocain were injected; no adrenalin was used. Sudden cyanosis, fell forward unconscious; artificial respiration, pulmotor and stimulants to no avail. Life pronounced extinct after four hours. Necropsy: Edema and congestion of lungs and brain. No lymphoid hypertrophy. This was reported as a Novocain death. It is manifestly not a death from that drug, nor can we believe that 2 c.c. of a 1 per cent solution of Novocain could cause death. Another death is recorded as due to Procain that seems to me to be doubtful. Here are the facts:

Male, age 39. Had hypertension and nephritis but considered a safe risk. Tonsils badly infected. Tonsillectomy. Patient received hypodermic of morphin and atropin (amount not stated) one-half hour before operation. Semi-reclining position. 0.5 per cent solution of Novocain containing 4 drops of 1-1000 adrenalin solution to the ounce. 1 drachm injected in each tonsil. Both tonsils injected before beginning operation. Right tonsil removed without any difficulty. As the left tonsil was being removed patient suddenly had convulsion. Became cyanotic with dilatation of pupils and cessation of respiration. Artificial respiration. Patient began having shallow respirations lasting about five minutes, remaining cyanotic. Death soon followed. No autopsy.

An individual with chronic nephritis and high blood-pressure has succumbed after a small dose of a very weak solution of Novocain. In the opinion of the writer the drug may only be considered as a contributing cause at the most.

Summary

It is interesting to note the ages of these patients; they were, 14-17-20-22-34-39, respectively, an average of 24½ years, indicating a susceptibility of the young.

A note of warning against the combination of cocain with other local anesthetics except in the smallest doses of the former must be sounded. At the risk of reiteration, the method of using unmeasured quantities of cocain solution should be discontinued. It is unscientific as well as unfair to our patients. The use of a minim measure will enable us at least to say that of a certain number of minims there remained so many, consequently not more than so many minims were used and the amount can then be fairly approximated. That certain people have an idiosyncrasy to cocain is no doubt true.

We are in hopes that a continued study of these fatalities will throw some further light on this question, and have agreed to serve on the permanent committee at our own expense in the hope of receiving further reports and that the profession will cooperate by publishing the facts or informing us of any deaths they hear of.

All communications will be considered confidential.

We are grateful for the many kindly words of approval and sincerely trust that by our labors we may prevent the waste of human life. We have demonstrated that it is possible to secure information as to medical matters by certain methods and that conclusions may be reached by following carefully formulated plans as in an engineering problem. In this instance we believe that we have demonstrated that it is possible for the physician without laboratory training to present a scientific study of a given subject, offering a constructive report with such recommendations as will render safer and more effective one of the most valuable and important aids to our therapy.

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Colonel Henry Clark Coe, M.D.

It is a rare privilege to add to the list of Contributing Editors of THE MEDICAL TIMES, to fill an existing vacancy, the name of Colonel Henry Clark Coe, Medical Reserve Corps, U. S. Army, and one of New York's most distinguished obstetricians and gynecologists.

Col. Coe is a very living figure in the American world of medicine. For forty years he has ornamented his profession, standing in the very forefront.

A graduate of Yale, and a classmate and fraternity mate of former President Taft, he took his first medical degree at Harvard in 1881 and a similar degree at the College of Physicians and Surgeons of New York in 1882.

Col. Coe's professional experience is too well known to the rank and file to necessitate publishing. For many years he has been one of the most active members of the medical staffs of some of the largest hospitals. He holds the distinction of having been the first physician appointed to the Medical Reserve Corps by President Taft in 1911, when the Corps was established. He established and was the first president of the Medical Reserve Corps Association of the United States and it was due to his initiative, energy and vision that the U. S. Army had a small but effective body of medical reservists when war broke out.

Interested as he was in military affairs and after having spent much time with troops as a welcome medical guest, Col. Coe served actively as an officer

at the time of the Connecticut maneuvers in 1912, the Blue and Gray Reunion at Gettysburg in 1913, and at various schools of instruction at Tobyhanna, Pa. Upon the outbreak of hostilities, he went into active service and was shortly promoted from a First-Lieutenancy to a Majority. The Colonel organized the Reserve Corps in New York City and was president of the examining board. Soon he was sent abroad in command of a mobile operating unit, and a Colonel in the Medical Corps of the Regular Army, upon returning from the Western front in July, 1918, told the writer, who at that time was in military service in this country, that on July 4th, 1918, he saw Colonel Coe in a dug-out immediately behind the lines, operating. The weather was exceedingly hot and the Colonel was attired in an operating apron, trousers and white shoes. He had been operating steadily for more than 30 hours, stopping only to take a cup of coffee occasionally.

After the Armistice this gallant officer returned to this country and was acclaimed as one of the medical heroes of the War. By a very delightful coincidence Colonel Coe and his four sons, all officers, were serving in France simultaneously.

Upon entering the Army he had resigned his active hospital connection and the professorial position at Bellevue which he had graced for many years. After returning to civil life this genial medical man gathered up the scattered threads and resumed practice. At the present time he is consulting gynecologist at Bellevue, General Memorial, Woman's, and Foundling Hospitals. He is a bright and shining example of patriotism and it would be well if the younger medical men of the country were to emulate his life and deeds.

Daedalus

Every physician should read J. B. S. Haldane's brilliant little book (you can get through it in an evening) "Daedalus, or Science and the Future." In one part of the book there are some extracts from an imaginary essay on the influence of biology on history during the 20th century such as might be supposed to be in the hands of university students 150 years hence. As an indication of how readable for a physician Haldane's book is we give the following daring and plausible extract:

It was in 1951 that Dupont and Schwarz produced the first ectogenetic child. As early as 1901 Heape had transferred embryo rabbits from one female to another, in 1925 Haldane had grown embryonic rats in serum for ten days, but had failed to carry the process to its conclusion, and it was not till 1940 that Clark succeeded with the pig, using Kehlmann's solution as medium. Dupont and Schwarz obtained a fresh ovary from a woman who was the victim of an airplane accident, and kept it living in their medium for five years. They obtained several eggs from it and fertilized them successfully, but the problem of the nutrition and support of the embryo was more difficult, and was only solved in the fourth year. Now that the technique is fully developed, we can take an ovary from a woman, and keep it growing in a suitable fluid for as long as twenty years, producing a fresh ovum each month, of which 90 per cent can be fertilized, and the embryos grown successfully for nine months, and then brought out into the air. Schwarz never got such good results, but the news of his first success caused an unprecedented sensation throughout the entire world, for the birthrate was already less than the deathrate in most civilized countries. France was the first country to adopt ectogenesis officially, and by 1968 was producing 60,000 children annually by this method. In most countries the opposition was far stronger, and was intensified by the Papal Bull "Nunquam prius audito," and the similar fetwa of the Khalif, both of which appeared in 1960.

Icarus

Bertrand Russell, in his stimulating little book "Icarus," suggests that eugenics, in a more distant future, may aim not only at eliminating undesired types, but at increasing desired types. He thinks that moral standards may alter so as to make it possible for one man to be the sire of a vast progeny by many different mothers. But he points out that all such ambitious plans are sure to go awry because men of science are prone to be unset by the fallacy which assumes that a reform inaugurated by such men would be administered as they would wish, by men similar in outlook to those who have advocated it, whereas, the truth is that a reform, once achieved, is handed over to the average citizen. So that the types which would be increased, under a eugenic program, would not be those desired by the present-day idealistic eugenicist, but rather the types desired by the average official or social arbiter. Prime Ministers, Bishops, and others whom the State would consider desirable might become the fathers of most of the succeeding generation. He thinks that if medical men had much to say about a eugenic program they would breed a subservient population—assuming that they had attained to sufficient knowledge of heredity to be able to determine, within limits, what sort of population they could have—convenient to rulers but incapable of initiative. He would seemingly prefer to trust nature, and does indeed intimate that nature did a very good job when she made Micawber—certainly not a desirable parent from the orthodox eugenic point of view—the father of Charles Dickens, and that she probably does this sort of thing oftener and better than any board of officials, however wise, could ever do it.

The Mongol in Our Midst

Dr. F. G. Crookshank ("The Mongol in Our Midst") believes that the frequent occurrence of ill-developed persons of Mongolian type, and particularly of Mongolian idiots, is a fact strictly relevant to all hypotheses of human origins and descent, and that attempts to classify imbeciles and idiots by their ethnic characters should be taken seriously. The human race is of three-fold origin; there are three irreducible stocks corresponding to the three types of Great Ape: the chimpanzee, the gorilla, and the orang. The chimpanzee reflects the Semite, the orang reflects the Mongol, and the gorilla reflects the Negro.

Although Crookshank's book is small, it adduces an amazing amount of data which he handles in a masterly manner.

There are also personages of exceptional abilities who betray Mongolian traits which Crookshank does not believe accidental. On pages 7 and 8 of his book he gives a striking description of the mandarin or Buddha-like Clemenceau, which reminds us very much of a sketch of La Follette in a recent issue of the *New York Times* in which his Oriental "make-up" was featured.

Crookshank finds in many cases of dementia precox striking homologies with the chimpanzee, especially in the matter of posture.

That there has been Mongolian infiltration of the European population as a consequence of invasion we can hardly doubt on historical grounds.

It is an atavistic, or cousinly, explanation that Crookshank submits. Perhaps Clemenceau's writ-

ing of Buddhist plays since his retirement possesses a special significance in the light of Crookshank's researches.

A Notable Work on Cancer

J. Ellis Barker's work on "Cancer: How it is Caused; How it can be Prevented," is an extremely able performance. If a well equipped physician had undertaken to turn out a work on cancer along the same lines, he would probably not have done any better. Barker is an English layman, but his father was a distinguished physician and he seems to possess a certain inherited (?) aptitude for medical matters. By profession he appears to be a statistician. His great power for marshalling masses of facts regarding cancer, analyzing them, and then completely rationalizing them, ranks him as an able scientist. He believes in a pre-cancerous condition and thinks that cancer can be prevented. In simple terms, it is due to chronic poisoning and vitamine starvation. There is at present no cure for cancer and there may never be a cure. As cancer is due to wrong living, the only true remedy for the disease is right living. Mr. Barker believes that if the reforms suggested by him were to be adopted, cancer might become as rare as is leprosy, which in the Middle Ages was general throughout Europe. Read Mr. Barker's book and you will agree with him that the adoption of such reforms would reduce, even if they did not abolish cancer; certainly they would reduce, or abolish, a great many other diseases of civilization which likewise spring from chronic poisoning and vitamine starvation.

We move that the honorary degree of *Medicinae Doctor* be conferred upon Mr. Barker.

The Pathological Novel

Some one has coined the term "The Pathological Novel," which seems to us to characterize very aptly a prevailing type of book.

The Pathological Novel, at its worst, is written for neurotic people by a neurotic, and its chief characters are neurotic. It is of limited worth as a true transcript of life, and can have but little meaning to normal people, or to the gods themselves.

Normal people seem to read these precious works in a spirit of amused wonderment, much as intellectual folks enjoy the most banal of movies.

The fact that some neurotic person proclaims his genius and alludes to himself as an artist means nothing.

What has been called the Pathological Novel should be read understandingly, when it will yield more fun than a box of monkeys.

The physician, particularly, should derive more recreation than anybody else from the reading of such books.

It is true that some of the alienists, like Dr. Joseph Collins, seem to take these works very solemnly, but we fancy that most physicians find the sex fantasies and ecstasies of their authors extremely funny.

Literary Psychoanalysis

The writer wondered why he had found the stories of Katherine Mansfield baffling or incomprehensible, and those of D. H. Lawrence humorous, until he was informed by several ladies that the former writer wrote for women, and, of course, being a woman, understood their souls and minds, while D. H. Law-

rence either had the mind of a woman or an uncanny and extremely rare insight into those mysteries of mysteries to a mere man, the mind and soul of woman.

Men, it seems, have always written for men, and it is only today that writers for women are emerging.

Since the writer was so bluntly enlightened by the aforesaid ladies he confesses to a better understanding of the psychology of Katherine Mansfield's characters. Lawrence, however, he is not so sure about. When certain of his books were first scanned the conclusion (?) was reached that he was either a sick and neurotic man or a humorist who slyly, with his tongue in his cheek, was exploiting profitably our nasty Puritans—capitalizing cleverly but none too scientifically the Freudian lore. In his books the pornography-loving middle class could read about what they had little chance to do themselves, except subconsciously, done to the queen's taste by very competent fornicators representing their own types. Such scenes in D. L. Lawrence's books have always seemed extremely funny to the writer, always reminiscent of a cageful of lascivious monkeys performing comical acrobatics of sexual connotation, and while he is willing to admit that Lawrence deliberately caters to the suppressed Anglo-Saxon woman, the catering seems not to be done naturally, as it would be by a woman of Katherine Mansfield's literary power (supposing that she had ever developed themes in Lawrence's manner), but by a man whose insight, such as it is, is intellectual and commercial and not purely feminine or inspired.

It will be recalled that Otto Weininger, the young genius who committed suicide in his twenties, wrote a book in which he developed the thesis that there is a certain amount of femaleness in every man, and more or less maleness in every woman. But we do not have to invoke this hypothesis necessarily to explain a Lawrence.

Lawrence has apparently betrayed himself further in his recent work on American literary classics, in which he is frankly the humorist.

Katherine Mansfield does not have to be dealt with as does Lawrence, because she has a right, as a woman, to write for women in feminine fashion.

Lawrence is a deep 'un, but he may simply be a he-man pursuing a new literary dodge, far more brazen than any before attempted, which has completely fascinated the booboisie, and laughing in his sleeve while he gathers in the shekels.

P. S. Shown the foregoing remarks, the writer's lady friends declare that Lawrence is a sick man—TB, nervous wreck, and all the rest.

So the problem remains unsolved: Is Lawrence an artist or a clever merchant? An inspired seer or a thrifty humorist? One of God-given genius or a sick disseminator of sickly and false psychology?

Medical Courtesy

Let me begin by admitting frankly that I admire the younger generation of doctors, their wide and accurate information, their keenness, industry and enthusiasm. The surgeons possess a boldness and technical skill unknown to the famous ones of my day. The internists and specialists are fully up to date, the laboratory men furnish reports which can be relied upon, the X-ray workers are invaluable

aids to the diagnostician. Some times a practitioner of the old school may think that too much reliance is placed on the report of the pathologist and that operations are performed hastily (though successfully, as regards the safety of the patient) and without due consideration of the ultimate results. But, these too are checked up by the "follow up" clinics of our present standardized hospitals—one of the most important accomplishments of our American College of Surgeons.

I have no criticism to offer regarding the ability and "modernism" of the present practitioners, in fact I always choose a young consultant, which is the best proof of my confidence in them.

But,—I note with deep regret their commercial side.

The late Dr. T. Gaillard Thomas—once a name to conjure with, now only a memory—used to say: "Medicine is a noble profession, but a poor trade." What would he have thought of the present business methods of the profession? He was rather "hard-boiled" himself and most successful as a money-maker, but he would be left far behind in these days, when the golden calf seems to have replaced other gods.

Let us leave the material side and consider the ideal. After all it is only the intangible that we really possess, as one who has nearly reached the Psalmist's limit realizes only too keenly. I am a "modernist" in the higher sense of the word, but I still cling to "the ancient landmarks" until I find some better way. Youth is progressive, destructive of out-worn medical creeds and theories. I am watching eagerly to see what constructive work they will bring forth in the future,—not cynically, but hopefully.

When I read the copy of our new (or old?) "Medical Ethics," I said to myself: "How many of this generation will follow its rules?" Remembering the scrupulous practice of Jacobi, the elder Flint and all the honored physicians, whose portraits adorn the walls of the Academy of Medicine, a feeling of sadness came over me as I thought of the present "Zeit-Geist." As I look back over my forty years of medical activity, in which I have sought to cherish, imperfectly enough, the high ideals of all those fine gentlemen, who needed no code of ethics but the Golden Rule, I miss so many little courtesies which our profession seems to have forgotten. They still prevail in the legal profession and to some extent in the clerical, in spite of wide differences of opinion among the clergy. For example, it used to be the custom whenever a patient was sent to a consultant for the attending physician to either come with the patient, or to send a note stating briefly the facts in the case and the opinion desired. The consultant invariably returned the courtesy with an appreciative letter, giving his frank opinion, and there his connection with the case ceased unless he was again consulted by the attendant, or asked to perform the operation which he had advised. Now, too often a brief telephone message, or an address scribbled on a prescription-blank, is the only communication. Too often the patient comes of his, or her own accord and, with the prevailing disloyalty of the laity, only gives the name of the doctor after close questioning.

Personally I rather prefer to make my diagnosis unbiased by the opinions of former consultants, but

what I do deplore is this: When I write a courteous note to the family doctor (if the patient is sufficiently loyal to have one) I hate to have him absolutely ignore it, as is too often the case. It may be "business," but it is not good manners. The excuse "no time" is a joke among professional and business men. They all have time to do what they want to do. New York has grown to be a big, cruel city, where it is every man for himself. "Nothing succeeds like success" in our community. What is "success"? Is it money, reputation, a crowded consulting room, or does it mean something higher and finer?

I want the younger men to ask themselves this question seriously as they climb upward. When they come to realize the emptiness of the things which once seemed so important they will crave real friends,—friends who are not bound to them by the frail ties of self interest, but friends who love and respect them for what they are, or have been, whose hands they will still grasp across the shoreless sea after the dream of life has faded.

H. C. C.

Miscellany

Conducted by ARTHUR C. JACOBSON, M.D.

Doctors—Then and Now

By L. N. in *Toronto Star Weekly*

In the old days when I had a kink in my system,
I went to Dr. Jones.
And he'd tell me to say oh, and hit me a wallop in
the ribs,
And ask how the wife was, and how the kid was
getting along at high school,
And he'd write a prescription,
That tasted as bad as it looked,
And nearly as bad as it smelled.
But it did the trick. It would have put considerable
animation
Into one of those statues with the baggy trouser legs
Up in Queen's Park.
And then, a year or two afterwards, I'd pay his bill
After I'd paid every other darned thing
I owed.

* * * * *

Nowadays when I have a pain,
I take it to a specialist.
He has one of those hard-finished, white Valspar
nurses
That scares the heart out of me.
She puts me into a card index,
With the story of my past life, and my thumb prints.
Then she shows me into an inner room.
It's white, too, just like a cafeteria.
Only there's nothing to eat.
And the specialist gives me a cold glance, and says,
"Sit there."
And I sit.
Then he opens up his tool kit, and boils his tools a
while.
And when he thinks they're done enough,
He inserts them into my eye, ear, nose and throat.
When they make me squirm, he gives me a dirty look,
And I stop squirming.
At last he says he thinks he may be able to do some-
thing for me,

If I'll have my eye, ear, nose and throat cut out.
And I say I'll think about it.
You bet I will!
And I totter out the wrong door,
But that white, hard-finished nurse retrieves me,
And says, "That will be ten dollars, please."

* * * * *

Once I made a joke in a specialist's office.
It was a very little one.
But it fell on the sterilized floor with a loud crash.
The doctor picked it up with his forceps
And put it into a white enameled can, along with
some

Discarded tonsils.

I suppose it wasn't an antiseptic joke.

* * * * *

Anyhow, I wish Dr. Jones
Hadh't stopped practising.

Pain in Infectious Diseases

(Concluded from page 246)

must be directed by characteristics of the individual cases. The experience with such treatment is thoroughly favorable. Patients so treated present an unusually favorable picture, throughout the course of the disease. As a rule they do not have the cyanosis which usually accompanies pneumonia, their respiration is less rapid, and the patient rests quietly. The same experience is noted when this treatment is used in grip-pneumonia.

As to the objective symptoms, the results in patients treated in this manner show that the illness passes off easily, the crises are not so severe, often not being marked at all and in the case of weak heart, the threatening phenomena, characteristic of the fifth to seventh days, seldom appear. It is certain that neither morphin nor the pyramidon group have any influence whatsoever on cyanosis, on respiration or on the heart. They are only used to eliminate certain manifestations leading to excessive muscle strain which burden the heart. This muscle strain is caused by restlessness following pain, by irritating cough excitations and resulting complications.

Thus, in pneumonia, the treatment of the accompanying pain is directly related to the conservation of the heart's function.

It should be mentioned here that morphin administration should cease the moment fever is lowered. There have never been any difficulties observed from thus discontinuing its use.

To a certain degree, treatment in scarlet fever, in septic angina and in other septic diseases, with painful local manifestations, may be carried on similarly. The prognosis of all these diseases depends upon the condition of the heart. It is undoubtedly true that the restlessness which constantly persists, as has been described in the consideration of pneumonia, has the effect of straining the heart. While in pneumonia, pleural pain and cough irritation are prominent, in other diseases, infiltration of certain tissues are the causative factors of pain: glandular swelling in scarlet fever; the distressing condition of tonsils and glands in angina, diphtheria and Vincent's angina being examples in question. The pain in the swollen tissues of and about the throat excites a choking sensation, and there ensues increased swelling. Exterior cold applications, though helpful, seldom remove the pain sufficiently satisfactorily. Gargling with antineuralgic agents serves ultimately only

to increase irritation in severe cases. Here also the relief agent may infringe upon the treatment of the cause. On the other hand if pain is eliminated, the irritability of the swollen organ is decreased, and the danger of a blockage of the trachea is lessened. In such cases it is generally not necessary to administer morphin—an anelgesic agent is ordinarily quite sufficient. Here also one should not wait until the pain is severe, since accompanying and following pain increased swelling and irritation ensue; therefore such treatment should be begun at an early stage. In cases of severe acute disease, pain should be regarded as a serious symptom, the exaggeration of which will dangerously influence the course of the pathologic process.

The offtime intensive pain of erysipelas is similar in behavior to the pain previously noted. Erysipelas, like all streptococci infections, does not arise and continue on so much through direct bacterial influence as through toxic substances affecting the heart, so that in this group also the heart and its disturbed functions are prognostically important. In pneumonia there is overloading of the circulation; in erysipelas it is a question of the toxic influence of the excitants. Therefore every additional burden should be avoided, and pain creates such a burden. In both cases treatment of pain becomes necessary from the viewpoint of salvage of the heart's action.

In erysipelas, resort should not be had to the use of morphin, but the simpler pain relieving agents should be employed.

In cases of local infection pain is a lesser problem. In carbuncles, felons, infection of the gums, etc., treatment is dependent upon the individual, and the judgment of the physician should determine the need for pain alleviation.

Summary

The problem of the treatment of pain which has been briefly considered here in the cases of the most important infectious diseases, is closely related to the treatment of the cause of the pain. We must grant that as yet we know little about the causative treatment of acute infectious diseases. We have at our disposal the serum and other specific forms of treatment, and the non-specific protein therapy. In both of these the source of the disease is attacked; in one instance by the direct administration of antibodies, and in the other by increasing body resistance. In severe infections these means in themselves are not sufficient. Treatment of those organs, on whose undisturbed function a favorable prognosis essentially depends, is indispensable, and it may be noted from the foregoing that in acute infections the treatment of pain is closely related to the treatment of the original cause, and is, therefore, an important practical consideration for the physician.

Modern Pyelography

(Concluded from page 249)

Tuberculosis

For obvious reasons a pyelogram should not be made in a case of renal tuberculosis if it can be avoided. It is usually possible to make a diagnosis of kidney tuberculosis, by the finding of the tubercle bacilli in the kidney urine at least. Often the bacilli may be found by the anteformin concentration tests, or guinea pig inoculation. In a closed renal tuberculosis it may be necessary to do a pyelogram and in this disease, when it is done we get a quite typical picture. Typical pictures are marked in well ad-

vanced cases; and it is seldom we encounter early renal tuberculosis.

What are the usual pyelographic points of interest in this condition? Undoubtedly the most striking is the areas of necrosis often extending from the pelvis, well up into the cortex of the kidney. This often gives a ragged or better say moth eaten appearance and is noticeable fairly early. Dilated pelvis is usually present, along with tubercular disease of the ureter and resultant stenosis. A rather important point to remember is that tuberculosis of the kidney seems too often to be a silent process until the disease is fairly well advanced. The early symptoms as a rule being not severe enough to cause real annoyance. This is particularly so when the original areas are in the parenchyma of the organ.

Polycystic Kidney

This condition in the vast majority of cases can be diagnosed without a pyelogram. The large kidney sacks, usually bilateral, can be distinctly palpitated as a rule. In the pyelogram they usually appear like large multilocular cysts, with somewhat of a kidney shape, often extending from the pelvis, well up beneath the costal borders.

Renal Calculi

Pyelography makes possible the diagnosis of kidney stone in about 97 per cent of our cases. The remaining 3 per cent are of the xanthin and cystin types which are so fine that they as a rule give us very little clue, except when we find their fragments in the urine. All of the other forms of concretions cast a very definite shadow. Hunt tells us in a report based on the examination of eighteen hundred cases that only eight per cent of the stones are in the parenchyma of the kidney.* This is an important contribution, but even so it is still quite essential for us to know whether the stone is in the perianchyma or the pelvis, as the operative procedure is somewhat different in the two locations.

Difference in individual development may play a part in causing difficulties in diagnosis, but it will usually be found that a stone whose shadow appears to be decidedly mesial lies in the true kidney pelvis and can be removed by pyelotomy. If, however, the shadow lies within the kidney proper, a nephrotomy may be necessary for its removal. The intensity of the shadow is dependent on the chemical composition of the stone, the best shadows being cast by the calcium salts, carbonates, oxalates and phosphates in the order mentioned. Triple phosphates and uric acid are less intense and xanthin and cystin cast good shadows very seldom.

From the foregoing brief review of the roentgen technique aided by shadow producing substances introduced into the kidney pelvis, I believe it becomes clear that pyelography as it is now generally called has passed the trial period. This procedure when properly carried out is practically free of ordinary dangers; is of the utmost value in urological diagnosis and should be used wherever indicated.

229 West 101st Street.

* From the James Buchanan Brady Urological Foundation of The New York Hospital.

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Uterine Myomata (Concluded on page 251)

in young women. In the latter, the ovary is more resistant to the action of the rays and it may happen that menstruation may be reestablished after a temporary period of amenorrhea, even pregnancy may take place. So it was argued that there is a possibility of the birth of monstrosities due to injury of the ovum. Experience in a very large number of women who gave birth after they were so treated, however, has shown that this never happens. In fact, some workers purposely attempt to so regulate the dose as to produce a temporary amenorrhea. Theoretically, this is made possible by the biological fact that some of the premordial follicles which are at rest during the treatment have a greater power of resistance than those in the stage of active cell division and may survive the attack and even retain their function. This occasionally happens. Because of our limited knowledge of the biological action of the rays, the above facts are not of much value from a practical standpoint, as it is impossible to so regulate the dose as to produce the desired effect with any degree of certainty.

From the discussion of the indications and contra indications, it will be seen that a correct diagnosis is of prime importance in deciding the method of treatment to be chosen. To prevent failure and unpleasant consequences it is necessary for the roentgenologist to work hand in hand with competent gynecologist as well as to be thoroughly equipped for his work.

Report of Cases.

This report comprises a period of one year, during which time eleven cases of fibromyomata of the uterus were treated, ten with *x*-rays and one with radium inserted into the cavity of the uterus. This was done at the urgent request of the attending physician, although both physician and patient were warned about the possibility of failure. The patient, a woman about thirty-five years of age, had a tumor of the fundus the size of which was that of a small orange. She bled profusely off and on for ten weeks. She received nine hundred milligram-hours of radium. She then developed nausea and vomiting which became so severe that the treatment had to be discontinued. The result was a complete failure. The patient, in her disappointment, began to make the rounds. Unfortunately for herself, she selected a laboratory equipped with a diagnostic machine and the results of a course of *x*-ray treatment was no better than with radium.

Of the ten patients treated with *x*-rays, nine became amenorrheic after one course of treatment and in one a second course had to be given. This patient had a large tumor of the uterus and the position of the ovaries could not be determined. In these cases it is always advisable to somewhat increase the dose on account of a possible increased depth of the ovary.

Out of the other nine patients, in six the course was administered in one sitting and in the balance the course had to be stretched over a period of several days. The chief reason for this procedure was obesity. One of this group was extremely corpulent and weighed over three hundred pounds. Every available means of increasing the intensity of the deep dose had to be utilized, the duration of the treatment was four hours. The effort was crowned with success. She became amenorrheic and remained so to date.

The patients ranged in age from twenty-nine to forty-four, the youngest one among them was a woman suffering from pulmonary tuberculosis which was complicated by uterine fibroids. The repeated hemorrhages pulled her down to such an extent that operation was out of question. As she was a very thin patient, she received the entire course in one sitting and became amenorrheic after it. None of the patients suffered any constitutional or local ill effects from the treatment. The physicians who have referred these cases all reported marked shrinkage of the tumors, in some instances as early as two weeks after treatment. The duration of a course of treatment in most instances was from one to one and one-half hours, working with 200,000 volts, 4 milliamperes, a filter of 0.5 mm. cu. + 2 mm. al. — at a distance of 33 centimeters.

One case, not included in the above group, well illustrates the necessity of a thorough diagnosis. The patient gave a history of metrorrhagia of fifteen years standing. Her case was diagnosed as that of myoma of the fundus on several occasions and she was referred to me for *x*-ray castration. On careful examination, however, an early carcinoma of the cervix was found in addition to the myoma. She was a very obese patient and suffered from severe secondary anemia. The surgeon disadvised operation and she received a combined course of radium and *x*-rays. The response was phenomenal. Only six weeks after the first course of treatment, there was no evidence of the previously existing cervical tumor, the hemorrhages had entirely ceased, and her blood count changed from one and one-half million to four and one-half millions. At this time, a year after her first treatment, there is no evidence of any recurrence. Had this patient been treated for fibroids with an insufficient dose, the result might have been disastrous.

1955 Grand Concourse.

Medico Legal Relations (Continued from page 257)

which is not at all uncommon, had unbalanced an unstable temperament. His friends feared that it would be impossible to take him to an institution except by force; and the other doctor in the case had already made out and signed the commitment papers. I declined to sign, because I do not care to commit if any other way presents itself, as in many instances it is as bad a handicap in future life as a prison sentence. It is a legal act; it remains forever a blot and a blemish upon a family. Voluntary commitment does not. By a little persuasion I induced this man to so commit himself. He soon became a model patient, and enjoyed the new environment so well that he wanted to try out two or three other institutions, which he did. He was under observations in institutions of his own choosing for over six months, and finally returned to his work. It is now nearly four years and he has had no return of attacks. This man was saved the ignominy and disgrace of being committed to an institution where he might have remained indefinitely.

(Concluded over the next page)